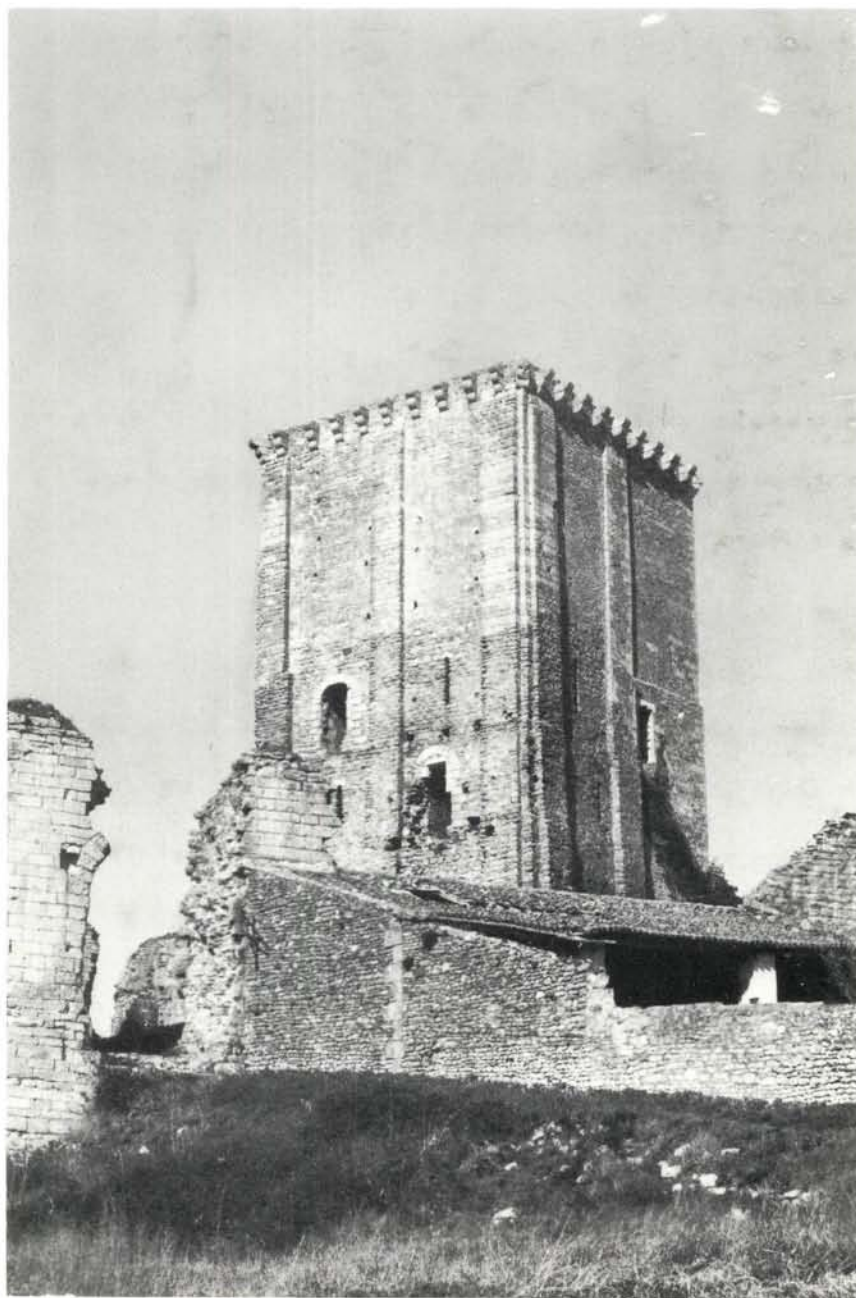


VOLUME 88 • NUMBER 3 • JUNE 1983

The American Historical Review

AMERICAN HISTORICAL ASSOCIATION



Oxford

Between the Guillotine and Liberty **Two Centuries of the Crime Problem in France**

Gordon Wright, William H. Bonsall Professor of History Emeritus, Stanford University. "Like everything Gordon Wright does, this book is wonderfully synthetic, humane, and elegantly written. . . . It gives us a very solid and coherent background against which to place our more specialized work, and it provides an interpretive framework with which we can debate."—Robert A. Nye, University of Oklahoma
1983 288 pp. \$19.95

Justice Without Law

Jerold S. Auerbach, Professor of History, Wellesley College. The first history of dispute settlement in the United States. "Lively and penetrating. . . . It provides an invaluable historical vantage for understanding current proposals for alternatives to law."
—Marc Galanter, University of Wisconsin
1983 224 pp. \$16.95

Booker T. Washington **The Wizard of Tuskegee, 1901-1915**

Louis R. Harlan, Professor of History, University of Maryland. A sequel to Harlan's Bancroft Prizewinning biography of Washington, *The Making of a Black Leader, 1856-1901*, this volume completes one of the most significant biographies of this generation. "Virtually flawless, always absorbing."—August Meier, Kent State University.
1983 540 pp., 20 illus. \$30.00

America's Quest for the Ideal Self **Dissent and Fulfillment in the 60s and 70s**

Peter Clecak, Professor of Social Thought and Comparative Culture, University of California, Irvine. Pointing out the remarkable unities between the decades instead of the usual disjunctures, Clecak argues that the American experience of the sixties and seventies can best be seen as a many-sided quest for personal salvation and social justice.
1983 368 pp. \$27.50

Martin Van Buren **The Romantic Age of American Politics**

John Niven, Professor of History, Claremont Graduate School. "The Little Magician has at last found a biographer worthy of his stature and significance in American history. This is a masterful study of a much underestimated statesman. It is meticulously researched and beautifully crafted."
—Robert V. Remini, University of Illinois
1983 736 pp., 50 illus. \$35.00

The Politics of Recovery **Roosevelt's New Deal**

Albert U. Romasco, Professor of History, New York University. Romasco sheds new light on the political factors and pressure groups that shaped the evolving New Deal. "A brilliant analysis of how and why Franklin Roosevelt's New Deal recovery measures could be economic failures yet political successes."—Ellis W. Hawley, University of Iowa
1983 320 pp. \$19.95

OXFORD UNIVERSITY PRESS
200 Madison Avenue, New York, New York 10016

The American Historical Review

AMERICAN HISTORICAL ASSOCIATION

Founded in 1884. Chartered by Congress in 1889

Elected Officers

President: PHILIP D. CURTIN, *Johns Hopkins University*

President-elect: ARTHUR S. LINK, *Princeton University*

Vice-Presidents: MARY F. BERRY, *Howard University and U.S.*

Commission on Civil Rights, Professional Division

JOHN A. GARRATY, *Columbia University, Teaching Division*

GERHARD L. WEINBERG, *University of North Carolina, Chapel Hill,*
Research Division

Appointed Officers

Executive Director: SAMUEL R. GAMMON

AHR Editor: OTTO PFLANZE, *Indiana University*

Controller: JAMES H. LEATHERWOOD

Elected Council Members

GORDON A. CRAIG, *Stanford University*

Immediate past President

JOYCE O. APPLEBY
University of California,
Los Angeles

ELIZABETH L. EISENSTEIN
University of Michigan,
Ann Arbor

FREDERIC E. WAKEMAN, JR.
University of California,
Berkeley

KATHERINE FISCHER DREW
Rice University

ROBERT I. ROTBERG
Massachusetts Institute
of Technology

ROBERT M. WARNER
Archivist of the
United States

Cover illustration: The Angevin tower of Moncontour in western France. Photograph, taken by Marco Trebbie, courtesy of Bernard S. Bachrach, University of Minnesota, Twin Cities. See his article in this issue, "The Angevin Strategy of Castle Building in the Reign of Fulk Nerra, 987–1040."

The American Historical Review appears in February, April, June, October, and December of each year. It is published by the American Historical Association, 400 A Street, S.E., Washington, D.C. 20003 and is printed and mailed by the William Byrd Press, 2901 Byrdhill Road, Richmond, Virginia 23228. The editorial offices are located in 914 Atwater, Indiana University, Bloomington, Indiana 47405.

The *AHR* is sent to members of the American Historical Association and to institutions holding subscriptions. Membership dues: For incomes over \$30,000, \$50.00 annually; \$20,000–\$29,999, \$42.00; \$15,000–\$19,999, \$35.00; \$10,000–\$14,999, \$25.00; below \$10,000 and joint memberships \$15.00; associate (non-historian) \$25.00; life \$1,000. The proportion of dues allocated to the *AHR* is \$17.00. Subscription rates effective for volume 87: Class I, *American Historical Review* only, United States, Canada, and Mexico \$43.00, foreign \$47.00. Further information on membership, subscriptions, and the ordering of back issues is contained on the two pages—1(a) and 2(a)—immediately preceding the advertisements.

Information concerning the submission of manuscripts and other matters of interest to authors and reviewers is contained on page 2(a), immediately preceding the advertisements.

Notice of nonreceipt of an issue must be sent to the Membership Secretary of the Association within three months of the date of publication of the issue. Changes of address should be sent to the Membership Secretary by the first of the month preceding the month of publication. The Association is not responsible for copies lost because of failure to report a change of address in time for mailing. Postmaster: Please send notification (Form 3579) regarding undelivered journals to: American Historical Association, 400 A Street, S.E., Washington, D.C. 20003. Publication identification number: *American Historical Review* (ISSN 0002-8762).

The Association cannot accommodate changes of address that are effective only for the summer months.

The *AHR* disclaims responsibility for statements, either of fact or opinion, made by contributors.

© AMERICAN HISTORICAL ASSOCIATION 1983

All rights reserved

Second-class postage paid at Washington, D.C., and at additional mailing offices

The American Historical Review

AMERICAN HISTORICAL ASSOCIATION

Editor: OTTO PFLANZE

Associate Editor: HELEN NADER

Assistant Editor: ANNE LEE BAIN

Acting Assistant Editor: CATHERINE ALBRECHT

Editorial Assistants: SHEILA A. CULBERT, SUE E. FACTOR, JAMES F. GOODE,
KAREN L. GATZ, RICHARD L. GAWTHROP, MICHELLE MANNERING,
JOHN O. NORMAN, PHILIP PAJAKOWSKI

Advertising Manager: IRA EUGENE CARREL

Board of Editors

WALTER L. ARNSTEIN
*University of Illinois,
Urbana-Champaign*

PAULINE R. MAIER
*Massachusetts Institute
of Technology*

LEONARD THOMPSON
Yale University

KATHERINE FISCHER DREW
Rice University

ALLEN MITCHELL
*University of California,
San Diego*

FREDERIC E. WAKEMAN, JR.
*University of California,
Berkeley*

JOHN HIGHAM
Johns Hopkins University

HANS J. ROGGER
*University of California,
Los Angeles*

HAROLD WOODMAN
Purdue University

SUSAN M. SOCOLOW
Emory University

Contributors:

BERNARD S. BACHRACH is a professor of history at the University of Minnesota, Twin Cities. He earned his Ph.D. at the University of California, Berkeley (1966), where he studied with F. L. Ganshof and Bryce Lyon. Among his publications are *Merovingian Military Organization* (1972), *A History of the Alans in the West* (1973), *Early Medieval Jewish Policy* (1977), and several articles with Dr. Jerome Kroll on the history of mental illness in the Middle Ages. Bachrach is also a cofounder and co-editor of the semi-annual journal *Medieval Prosopography*. He is currently pursuing several research projects that focus on society and the military in pre-Crusade Europe, and he recently conducted a seminar on archaeology and history at the École des Hautes Études (6th Section) and delivered a paper entitled "Animals and Warfare in Early Medieval Europe" at the 31st Settimana di Studio held at the Centro Italiano di Studi sull'alto Medioevo in Spoleto.

GENE BRUCKER, Shepard Professor of History at the University of California, Berkeley, received the A.B. and M.A. degrees from the University of Illinois, B.Litt. from Oxford University, and Ph.D. from Princeton University. His publications include *Florentine Politics and Society, 1343–1378* (1962), *Renaissance Florence* (1969), and *The Civic World of Early Renaissance Florence* (1977). He is currently on leave, doing research and writing at the Villa I Tatti in Florence.

ROGER FLETCHER is head of the history department at Meriden College, Sydney, Australia. He has just completed *Revisionism and Empire* (forthcoming) and is the author of

numerous articles on German revisionism in the Wilhelmine era, including "A Revisionist Looks at Imperialism: Eduard Bernstein's Critique of Imperialism and *Kolonialpolitik*, 1900–1914," *Central European History* (1979). His main areas of interest are the philosophy of international relations and the social history of foreign policy.

JON JACOBSON is an associate professor of history at the University of California, Irvine. He studied with Raymond J. Sontag at the University of California, Berkeley, where he received his Ph.D. in 1965. He is the author of *Locarno Diplomacy: Germany and the West, 1925–1929* (1972), which received the George Louis Beer Prize of the American Historical Association. He is currently doing research on the renegotiation of the World War I peace settlement during the 1920s.

NIKKI R. KEDDIE, a professor of history at the University of California, Los Angeles, and past president of the Middle East Studies Association of North America, has edited five books and authored, in addition to numerous articles, *Roots of Revolution: An Interpretive History of Modern Iran* (1981), *Iran: Religion, Politics, and Society* (1978), *Sayyid Jamal ad-Din "al-Afghani"* (1972), *An Islamic Response to Imperialism* (1968), and *Religion and Rebellion in Iran* (1966). She has traveled extensively in Europe, North Africa, and Asia, has had two exhibits of her Middle Eastern photographs, and has held numerous fellowships, most recently from the Rockefeller Foundation and the Woodrow Wilson International Center for Scholars, Washington, D.C.

Contents

VOLUME 88 • NUMBER 3 • JUNE 1983

Articles

- The Angevin Strategy of Castle Building in the Reign of Fulk Nerra,
987–1040, BY BERNARD S. BACHRACH 533
- Cobden as Educator: The Free-Trade Internationalism of
Eduard Bernstein, 1899–1914, BY R. A. FLETCHER 561
- The Iranian Revolution in Comparative Perspective,
BY NIKKI R. KEDDIE 579

Review Essays

- Tales of Two Cities: Florence and Venice in the Renaissance,
BY GENE BRUCKER 599
- Is There a New International History of the 1920s?
BY JON JACOBSON 617

Reviews of Books

GENERAL

- KARL F. MORRISON, *The Mimetic Tradition of Reform in the West*. By Charles Trinkaus 646
- STEVEN J. DICK, *Plurality of Worlds: The Origins of the Extraterrestrial Life Debate from Democritus to Kant*.
By Brian P. Copenhaver 647
- HUBERT L. DREYFUS and PAUL RABINOW, *Michel Foucault: Beyond Structuralism and Hermeneutics*.
By Dominick LaCapra 648
- JOAN LEOPOLD, *Culture in Comparative and Evolutionary Perspective: E. B. Tylor and the Making of Primitive Culture*.
By Kenneth A. R. Kennedy 649
- JOHN A. ARMSTRONG, *Nations before Nationalism*.
By Ira M. Lapidus 650
- V. G. KIERNAN, *From Conquest to Collapse: European Empires from 1815 to 1960*. By Martin Wolfe 651
- AMOS PERLMUTTER, *Modern Authoritarianism: A Comparative Institutional Analysis*. By Michael Curtis 652

- TREVOR I. WILLIAMS, *A Short History of Twentieth-Century Technology, c. 1900–c. 1950*. By Thomas J. Knight 653
- LESTER S. KING, *Medical Thinking: A Historical Preface*.
By Todd L. Savitt 654

ANCIENT

- PETER KRENTZ, *The Thirty at Athens*.
By John J. Keaney 654
- E. T. SALMON, *The Making of Roman Italy*.
By William V. Harris 655
- PETER GREENHALGH, *Pompey: The Roman Alexander*; PETER GREENHALGH, *Pompey: The Republican Prince*.
By Eleanor G. Huzar 656
- RICHARD P. SALLER, *Personal Patronage under the Early Empire*.
By D. Brendan Nagle 657
- TONY HONORÉ, *Emperors and Lawyers*.
By Bruce W. Frier 658

- J. R. MARTINDALE. *The Prosopography of the Later Roman Empire*. Vol. 2, A.D. 395–527.
By M. T. W. Arnheim 659
- TIMOTHY D. BARNES. *The New Empire of Diocletian and Constantine*. By Thomas W. Africa 660
- MEDIEVAL
- ANNE LLEWELLYN BARSTOW. *Married Priests and the Reforming Papacy: The Eleventh-Century Debates*.
By Glenn W. Olsen 660
- T. H. LLOYD. *Alien Merchants in England in the High Middle Ages*. By John H. Munro 661
- JOHN GILLINGHAM. *The Wars of the Roses: Peace and Conflict in Fifteenth-Century England*. By J. R. Lander 661
- NORMAN MACDOUGALL. *James III: A Political Study*.
By Michael Altschul 662
- LUKE E. DEMAITRE. *Doctor Bernard de Gordon: Professor and Practitioner*. By Richard C. Dalcs 663
- JAN ROGOZIŃSKI. *Power, Caste, and Law: Social Conflict in Fourteenth-Century Montpellier*. By John Hinc Mundy 663
- BRIAN PATRICK MCGUIRE. *The Cistercians in Denmark: Their Attitudes, Roles, and Functions in Medieval Society*.
By John B. Freed 664
- JAN T. HALLENBECK. *Pavia and Rome: The Lombard Monarchy and the Papacy in the Eighth Century*.
By David S. Sefton 665
- A. JA. GUREVICH. *Problemy srednevekovoi narodnoi kul'tury* [Problems of Medieval Popular Culture].
By Marc Szeftel 666
- MODERN EUROPE
- WINFRIED BAUMGART. *Imperialism: The Idea and Reality of British and French Colonial Expansion, 1880–1914*; H. JOHN FIELD. *Toward a Programme of Imperial Life: The British Empire at the Turn of the Century*. By Bernard Semmel 667
- DIETRICH EICHHOLTZ and KURT GOSSWEILER, eds. *Faschismus Forschung: Positionen, Probleme, Polemik*.
By Michael H. Kater 668
- HANNES JÓNSSON. *Friends in Conflict: The Anglo-Icelandic Cod Wars and the Law of the Sea*. By Lynton K. Caldwell 669
- BARRETT L. BEER. *Rebellion and Riot: Popular Disorder in England during the Reign of Edward VI*. By Dale Hoak 670
- PETER LAKE. *Moderate Puritans and the Elizabethan Church*.
By C. H. George 670
- PETER HOLMES. *Resistance and Compromise: The Political Thought of the Elizabethan Catholics*.
By Arnold Pritchard 671
- DAVID S. KATZ. *Philo-Semitism and the Readmission of the Jews to England, 1603–1655*. By S. D. Temkin 672
- AUSTIN WOOLRYCH. *Commonwealth to Protectorate*.
By Lawrence Kaplan 672
- REED BROWNING. *Political and Constitutional Ideas of the Court Whigs*. By Robert A. Smith 673
- J. C. D. CLARK. *The Dynamics of Change: The Crisis of the 1750s and English Party Systems*. By James L. McKelvey 674
- IAN R. CHRISTIE. *Wars and Revolutions: Britain, 1760–1815*; FRANK O'GORMAN. *The Emergence of the British Two-Party System, 1760–1832*. By Carl B. Cone 675
- P. J. CORFIELD. *The Impact of English Towns, 1700–1800*.
By Alan Dyer 676
- PAUL SAUNDERS. *Edward Jenner: The Cheltenham Years, 1795–1823; Being a Chronicle of the Vaccination Campaign*.
By Michael J. Durey 676
- KENNETH BOURNE. *Palmerston: The Early Years, 1784–1841*.
By Charles R. Middleton 677
- MARTIN PUGH. *The Making of Modern British Politics, 1867–1939*. By Peter Stansky 678
- HUGH A. MACDOUGALL. *Racial Myth in English History: Trojans, Teutons, and Anglo-Saxons*.
By Arthur B. Ferguson 679
- RICHARD ALLEN SOLOWAY. *Birth Control and the Population Question in England, 1877–1930*. By Andrew Rosen 680
- F. M. L. THOMPSON, ed. *The Rise of Suburbia*; DEREK FRASER, ed. *Municipal Reform and the Industrial City*.
By Roy Church 680
- STUART A. COHEN. *English Zionists and British Jews: The Communal Politics of Anglo-Jewry, 1895–1920*.
By Joseph Heller 682
- F. S. NORTHEGE and AUDREY WELLS. *British and Soviet Communism: The Impact of a Revolution*.
By Henry R. Winkler 682
- GEORGE R. HEWITT. *Scotland under Morton, 1572–80*.
By Charles H. Haws 683
- W. A. SPECK. *The Butcher: The Duke of Cumberland and the Suppression of the 45*. By P. W. J. Riley 684
- R. B. MCDOWELL and D. A. WEBB. *Trinity College, Dublin, 1592–1952: An Academic History*. By Harold Perkin 684
- JOSEPH V. O'BRIEN. "Dear, Dirty Dublin": *A City in Distress, 1899–1916*. By Anthony S. Wohl 685
- JOHN W. SHAFFER. *Family and Farm: Agrarian Change and Household Organization in the Loire Valley, 1500–1900*.
By George W. Grantham 686
- ROBERT DARNTON. *The Literary Underground of the Old Regime*. By Raymond Birn 687
- RAYMOND HUARD. *La préhistoire des partis: Le mouvement républicain en Bas-Languedoc, 1848–1881*.
By Katherine Auspitz 687
- GEORGE D. SUSSMAN. *Selling Mothers' Milk: The Wet-Nursing Business in France, 1715–1914*. By Edward Shorter 688
- PATRICIA O'BRIEN. *The Promise of Punishment: Prisons in Nineteenth-Century France*. By James F. Traer 689
- MICHEL LESCURE. *Les banques, l'état et le marché immobilier en France à l'époque contemporaine, 1820–1940*.
By Charles E. Freedeman 690
- STEPHEN WILSON. *Ideology and Experience: Antisemitism in France at the Time of the Dreyfus Affair*.
By Michael R. Marrus 691
- WILLIAM G. ANDREWS. *Presidential Government in Gaullist France: A Study of Executive-Legislative Relations, 1958–1974*.
By Steven Philip Kramer 692
- GUSTAV HENNINGSSEN. *The Witches' Advocate: Basque Witchcraft and the Spanish Inquisition (1609–1614)*.
By H. C. Erik Midelfort 692
- R. DAN RICHARDSON. *Comintern Army: The International Brigades and the Spanish Civil War*.
By David Wingate Pike 693
- JAN BRUNIUS. *Bondebygd i förändring: Bebyggelse och befolkning i västra Närke ca. 1300–1600* [A Rural Area in Transition:

- Settlement and Population in Western Närke, ca. 1300–1600]. By Jenny M. Jochens 694
- BRITT-MARIE LUNDBÄCK. *En industri kommer till stan: Hudiksvall och trävaruindustrin, 1855–1876* [An Industry Comes to Town: Hudiksvall and the Timber and Lumber Industry]. By Lasse Cornell 695
- JAMES CAVALLIE. *De höga officerarna: Studier i den svenska militära hierarkien under 1600-talets senare del*. [High-Ranking Officers: Studies on the Swedish Military Hierarchy in the Second Half of the Seventeenth Century]. By Heinz E. Ellersieck 695
- AARRE LÄNTINEN. *Kuninkaan "perintöä ja omaa" (arvi och eget): Kameraalihistoriallinen tutkimus Kustaa Vaasan maanmaisuuudesta Suomessa vuosina, 1531–1560* [The King's Inheritance and the King's Own: A Fiscal-Historical Study of Gustav Vasa's Landed Property in Finland, 1531–60]. By Anneli Mäkelä 696
- TUO MANNINEN. *Vapustaistelu, kansalaissota ja kapina: Taistelun luonne valkoisten sotapropagandassa vuonna 1918* [War of Liberation, Civil War, and Revolt: The Nature of the Conflict in White War Propaganda in 1918]. By Anthony F. Upton 697
- A. D. WRIGHT. *The Counter-Reformation: Catholic Europe and the Non-Christian World*. By Robert Bireley 698
- HARRO HÖPFL. *The Christian Polity of John Calvin*. By Robert M. Kingdon 698
- JAMES O'HIGGINS. *Yves de Vallome: The Making of an Esprit-Fort*. By Henry Heller 699
- ETIENNE FRANÇOIS. *Koblenz im 18. Jahrhundert: Zur Sozial- und Bevölkerungsstruktur einer deutschen Residenzstadt*. By Claus-Peter Clasen 700
- ISABEL V. HULL. *The Entourage of Kaiser Wilhelm II, 1888–1918*. By Lamar Cecil 700
- JOHN C. G. RÖHL and NICOLAUS SOMBART, eds. *Kaiser Wilhelm II: New Interpretations*. By Geoff Eley 701
- KONRAD H. JARAUSCH. *Students, Society, and Politics in Imperial Germany: The Rise of Academic Illiberalism*. By Walter Struve 702
- GERHARD BESIER. *Krieg—Frieden—Abrüstung: Die Haltung der europäischen und amerikanischen Kirchen zur Frage der deutschen Kriegsschuld, 1914–1933. Ein kirchenhistorischer Beitrag zur Friedensforschung und Friedenserziehung*. By Richard Gutteridge 703
- EBERHARD SCHANBACHER. *Parlamentarische Wahlen und Wahlsystem in der Weimarer Republik: Wahlgesetzgebung und Wahlreform im Reich und in den Ländern*. By Dieter K. Buse 704
- ROBERT WISTRICH. *Who's Who in Nazi Germany*. By Donald M. McKale 704
- RICHARD F. HAMILTON. *Who Voted for Hitler?* By Dietrich Orlow 705
- DENNIS E. SHOWALTER. *Little Man, What Now? Der Stürmer in the Weimar Republic*. By Gary D. Stark 706
- DIETER OSE. *Entscheidung im Westen, 1944: Der Oberbefehlshaber West und die Abwehr der alliierten Invasion*. By Earl F. Ziemke 707
- ALBERTO GROHMANN. *Città e territorio tra medioevo ed età moderna: Perugia, secc. XIII–XVI*. Vol. 1, *La città*; vol. 2, *Il territorio*. By William M. Bowsky 707
- CARLO PONI. *Fossi e cavedagne benedicon le campagne*. By Rudolph M. Bell 708
- A. ENZO BALDINI. *Puntigli spagnoleschi e intrighi politici nella Roma di Clemente VIII: Girolamo Prachetta e la sua relazione del 1603 sui cardinali*. By Judith Hook 709
- COSIMO SEMERARO. *Restaurazione chiesa e società: La "seconda ricupera" e la rinascita degli ordini religiosi nello Stato pontificio, Marche e Legazioni, 1815–1823*. By Alan J. Reinerman 709
- ROMANO UGOLINI. *Garibaldi: Genesi di un mito*. By Clara M. Lovett 710
- KARL A. ROIDER, JR. *Austria's Eastern Question, 1700–1790*. By Barbara Jelavich 711
- ALEXANDER S. MITRAKOS. *France in Greece during World War I: A Study in the Politics of Power*. By Jan Karl Tanenbaum 711
- RICHARD C. FRUCHT. *Dunarea Noastra: Romania, the Great Powers, and the Danube Question, 1914–1921*. By Sherman D. Spector 712
- JOSEF HARNA et al. *Materiály k politickým, hospodářským a sociálním dějinám Československa v letech 1918–1929* [Materials for the Political, Economic, and Social History of Czechoslovakia in the Years 1918–29]. By Victor S. Mamatey 712
- ROMAN SZPORLUK. *The Political Thought of Thomas G. Masaryk*. By Bruce M. Garver 713
- KÁLMAN JANICS. *Czechoslovak Policy and the Hungarian Minority, 1945–1948*. By Stanislav Kirschbaum 714
- WILLIAM M. BATKAY. *Authoritarian Politics in a Transitional State: István Bethlen and the Unified Party in Hungary, 1919–1926*. By Sandor Szilassy 715
- YISRAEL GUTMAN. *The Jews of Warsaw, 1939–1943: Ghetto, Underground, Revolt*. By Randolph L. Braham 716
- V. L. IANIN. *Novgorodskaja feodal'naja votchina: Istorikogenealogičeskoe issledovanie* [The Novgorodian Feudal Votchina: A Historical-Genecalogical Investigation]. By Daniel H. Kaiser 717
- ANDREAS KAPPELER. *Russlands erste Nationalitäten: Das Zarenreich und die Völker der Mittleren Wolga vom 16. bis 19. Jahrhundert*. By Alan Fisher 718
- E. I. DRUZHININA. *Iuzhnaia Ukraina v period krizisa feodalizma, 1825–1860 gg.* [The Southern Ukraine in the Period of the Crisis of Feudalism, 1825–1860]. By Stephan M. Horak 718
- E. N. BABIKOVA. *Dvoevlastie v Sibiri* [Dual Power in Siberia]. By Canfield F. Smith 719
- L. V. IVANOVA. *Formirovanie sovetskoi nauchnoi intelligentsii, 1917–1927 gg.* [The Formation of the Soviet Scholarly Intelligentsia, 1917–27]. By Loren R. Graham 720
- I. I. MINTS and A. P. NENAROKOV, eds. *Zhenshchiny—Revolutsionery i uchenye* [Women Revolutionaries and Scholars]. By Barbara Evans Clements 721

NEAR EAST

- MALCOLM CAMERON LYONS and D. E. P. JACKSON. *Saladin: The Politics of the Holy War*. By Carl F. Petry 721
- GABRIEL BAER. *Fellah and Townsman in the Middle East: Studies in Social History*. By Arnold H. Green 723
- MANGOL BAYAT. *Mysticism and Dissent: Socioreligious Thought in Qajar Iran*. By Peter Chelkowski 723
- HAMID ENAYAT. *Modern Islamic Political Thought*. By Caesar E. Farah 724
- ALAN R. TAYLOR. *The Arab Balance of Power*. By Philip S. Khoury 725

AFRICA

- J. DESMOND CLARK, ed. *The Cambridge History of Africa*. Vol. 1, *From the Earliest Times to c. 500 B.C.* By Merrick Posnansky 726
- MORDECHAI ABIR. *Ethiopia and the Red Sea: The Rise and Decline of the Solomonic Dynasty and Muslim-European Rivalry in the Region*. By James Kirkman 727
- PATRICK MANNING. *Slavery, Colonialism, and Economic Growth in Dahomey, 1640–1960*. By John D. Hargreaves 727
- MARC MICHEL. *L'appel à l'Afrique: Contributions et réactions à l'effort de guerre en A.O.F., 1914–1919*. By William B. Cohen 728
- T. O. BEIDELMAN. *Colonial Evangelism: A Socio-Historical Study of an East African Mission at the Grassroots*. By F. K. Ekechi 729
- PHYLLIS LEWSEN. *John X. Merriman: Paradoxical South African Statesman*. By Andrew Porter 730
- JOHN WESTERN. *Outcast Cape Town*. By Sheridan Johns 731

ASIA AND THE EAST

- JEROME CH'EN. *State Economic Policies of the Ch'ing Government, 1840–1895*. By Wellington K. K. Chan 731
- ROBERT L. IRICK. *Ch'ing Policy toward the Coolie Trade, 1847–1878*. By Peter Ward Fay 732
- R. KEITH SCHOPPA. *Chinese Elites and Political Change: Zhejiang Province in the Early Twentieth Century*. By Mary Backus Rankin 733
- PAUL HENG-CHAO CH'EN. *The Formation of the Early Meiji Legal Order: The Japanese Code of 1871 and Its Chinese Foundation*. By Richard H. Minear 733
- WILLIAM MILES FLETCHER III. *The Search for a New Order: Intellectuals and Fascism in Prewar Japan*. By George Macklin Wilson 734
- THOMAS R. H. HAVENS. *Artist and Patron in Postwar Japan: Dance, Music, Theater, and the Visual Arts, 1955–1980*. By Hugo Munsterberg 735
- TAPAN RAYCHAUDHURI and IRFAN HABIB, eds. *The Cambridge Economic History of India*. Vol. 1, c. 1200–c. 1750. By Hamida Khatoun Naqvi 735
- N. GERALD BARRIER, ed. *The Census in British India: New Perspectives*. By Blair B. Kling 737
- GAIL MINAULT. *The Khilafat Movement: Religious Symbolism and Political Mobilization in India*. By Stephen F. Dale 737
- ROBERT GRANT IRVING. *Indian Summer: Lutyens, Baker, and Imperial Delhi*. By J. Mordaunt Crook 738
- MILAN HAUNER. *India in Axis Strategy: Germany, Japan, and Indian Nationalists in the Second World War*. By Charles B. Burdick 739
- ULF SUNDHAUSSEN. *The Road to Power: Indonesian Military Politics, 1945–1967*. By Justus M. Van der Kroef 740
- JAMES PINCKNEY HARRISON. *The Endless War: Fifty Years of Struggle in Vietnam*. By George C. Herring 740
- KAY SAUNDERS. *Workers in Bondage: The Origins and Bases of Unfree Labour in Queensland, 1824–1916*. By L. L. Robson 741
- STEPHEN HOLT. *Manning Clark and Australian History, 1915–1963*. By Kay Saunders 742

UNITED STATES

- ROBERT V. WELLS. *Revolutions in Americans' Lives: A Demographic Perspective on the History of Americans, Their Families, and Their Society*. By Carl N. Degler 743
- BRIAN W. DIPPIE. *The Vanishing American: White Attitudes and U.S. Indian Policy*. By Francis Paul Prucha 744
- NATHAN O. HATCH and MARK A. NOLL, eds. *The Bible in America: Essays in Cultural History*. By Martin E. Marty 744
- DANIEL J. CZITROM. *Media and the American Mind: From Morse to McLuhan*. By Garth S. Jowett 745
- RICHARD L. RUBIN. *Press, Party, and Presidency*. By George Juergens 746
- NEAL SALISBURY. *Manitou and Providence: Indians, Europeans, and the Making of New England, 1500–1643*. By Bernard W. Sheehan 746
- DOROTHY V. JONES. *License for Empire: Colonialism by Treaty in Early America*. By John Phillip Reid 747
- DAVID B. QUINN, ed. *Early Maryland in a Wider World*. By Gloria L. Main 748
- LOUIS W. POTTS. *Arthur Lee: A Virtuous Revolutionary*. By Charles Royster 748
- CHARLES H. LIPPY. *Seasonable Revolutionary: The Mind of Charles Chauncy*. By George Selement 749
- MARY BETH NORTON. *Liberty's Daughters: The Revolutionary Experience of American Women, 1750–1800*. By Lyle Koehler 750
- LAWRENCE DELBERT CRESS. *Citizens in Arms: The Army and the Militia in American Society to the War of 1812*. By John Pancake 751
- REGINALD C. STUART. *War and American Thought: From the Revolution to the Monroe Doctrine*. By George A. Billias 752
- MARY P. RYAN. *Cradle of the Middle Class: The Family in Oneida County, New York, 1790–1865*. By Robert A. Gross 752
- BERTRAM WYATT-BROWN. *Southern Honor: Ethics and Behavior in the Old South*. By J. Mills Thornton III 753
- MARK E. NEELY, JR. *The Abraham Lincoln Encyclopedia*. By Thomas R. Turner 754
- PHYLLIS F. FIELD. *The Politics of Race in New York: The Struggle for Black Suffrage in the Civil War Era*. By Louis S. Gerteis 755
- E. B. LONG. *The Saints and the Union: Utah Territory during the Civil War*. By Roger D. Launius 756
- WILLIAM W. SAVAGE, JR. *The Cowboy Hero: His Image in American History and Culture*. By Edwin R. Bingham 756
- RONALD H. LIMBAUGH. *Rocky Mountain Carpetbaggers: Idaho's Territorial Governors, 1863–1890*. By Clark C. Spence 757
- THOMAS J. NOEL. *The City and The Saloon: Denver, 1858–1916*. By Gunther Barth 757
- HENRY C. DETHLOFF and IRVIN M. MAY, JR., eds. *Southwestern Agriculture: Pre-Columbian to Modern*. By Thomas D. Isern 758
- STEPHEN J. PYNE. *Fire in America: A Cultural History of Wildland and Rural Fire*. By Roderick Nash 759
- CRAIG W. ALLIN. *The Politics of Wilderness Preservation*; ROBERT W. RIGHTER. *Crucible for Conservation: The Creation of Grand Teton National Park*. By Thomas G. Alexander 759
- MARILYN TOBIAS. *Old Dartmouth on Trial: The Transformation*

<i>of the Academic Community in Nineteenth-Century America.</i> By Frederick Rudolph	760	FRANCISCO E. BALDERRAMA. <i>In Defense of La Raza: The Los Angeles Mexican Consulate and the Mexican Community, 1929 to 1936.</i> By Thomas Schoonover	774
FREDERICK C. DAHLSTRAND. <i>Amos Bronson Alcott: An Intellectual Biography.</i> By Robert H. Abzug	761	KARAL ANN MARLING. <i>Wall-to-Wall America: A Cultural History of Post-Office Murals in the Great Depression.</i> By H. Wayne Morgan	775
ROBERT M. CRUNDEN. <i>Ministers of Reform: The Progressives' Achievement in American Civilization, 1889-1920.</i> By David W. Noble	762	CHRISTOPHER J. KAUFFMAN. <i>Faith and Fraternalism: The History of the Knights of Columbus, 1882-1982.</i> By Norbert C. Brockman	775
SUSAN TIFFIN. <i>In Whose Best Interest? Child Welfare Reform in the Progressive Era;</i> W. NORTON GRUBB and MARVIN LAZERSON. <i>Broken Promises: How Americans Fail Their Children.</i> By Jeremy P. Felt	762	MAURICE ISSERMAN. <i>Which Side Were You On? The American Communist Party during the Second World War.</i> By Kenneth E. Hendrickson, Jr.	776
DAVID ROSNER. <i>A Once Charitable Enterprise: Hospitals and Health Care in Brooklyn and New York, 1885-1915.</i> By John Duffy	763	LEONARD MOSLEY. <i>Marshall: Hero for Our Times.</i> By Warren I. Cohen	776
HARRY F. DOWLING. <i>City Hospitals: The Undercare of the Underprivileged.</i> By Judith Walzer Leavitt	764	G. EDWARD WHITE. <i>Earl Warren: A Public Life.</i> By Michal R. Belknap	777
GEORGE JUERGENS. <i>News from the White House: The Presidential-Press Relationship in the Progressive Era.</i> By David Culbert	765		
ARTHUR S. LINK, ed. <i>Woodrow Wilson and a Revolutionary World, 1913-1921.</i> By N. Gordon Levin, Jr.	765		
YVES-HENRI NOUAILHAT. <i>France et États-Unis, août 1914-avril 1917.</i> By Stephen A. Schuker	766		
LESLIE FISHBEIN. <i>Rebels in Bohemia: The Radicals of The Masses, 1911-1917.</i> By Robert A. Rosenstone	767		
WILLIAM H. HARRIS. <i>The Harder We Run: Black Workers since the Civil War.</i> By Donald Spivey	768		
ARNOLD SHANKMAN. <i>Ambivalent Friends: Afro-Americans View the Immigrant.</i> By Christine Bolt	768		
JOSEPH P. SCHULTZ, ed. <i>Mid-America's Promise: A Profile of Kansas City Jewry;</i> WILLIAM TOLL. <i>The Making of an Ethnic Middle Class: Portland Jewry over Four Generations.</i> By Lloyd P. Gartner	769		
DANIEL SCHAEFFER. <i>Garden Cities for America: The Radburn Experience.</i> By William B. Scott	770		
RONALD D. ELLER. <i>Miners, Millhands, and Mountaineers: Industrialization of the Appalachian South, 1880-1930.</i> By Tom E. Terrill	771		
DANIEL JOSEPH SINGAL. <i>The War Within: From Victorian to Modernist Thought in the South, 1919-1945.</i> By Jon L. Wakelyn	772		
JAMES R. MCGOVERN. <i>Anatomy of a Lynching: The Killing of Claude Neal.</i> By Dan T. Carter	772		
JAMES H. MADISON. <i>The History of Indiana. Vol. 5, Indiana through Tradition and Change: A History of the Hoosier State and Its People, 1920-1945.</i> By Robert C. Nesbit	773		
		CANADA	
		DEREK PETHICK. <i>The Nootka Connection: Europe and the Northwest Coast, 1790-1795.</i> By Robert Erwin Johnson	778
		LATIN AMERICA	
		ENRIQUE DUSSEL. <i>A History of the Church in Latin America: Colonialism to Liberation, 1492-1979.</i> By Daniel H. Levin	778
		JACK A. LICATE. <i>Creation of a Mexican Landscape: Territorial Organization and Settlement in the Eastern Puebla Basin, 1520-1605.</i> By G. Michael Riley	779
		NORA HAMILTON. <i>The Limits of State Autonomy: Post-Revolutionary Mexico.</i> By Roderic A. Camp	780
		MILES L. WORTMAN. <i>Government and Society in Central America, 1680-1840.</i> By William L. Sherman	780
		RICHARD H. IMMERMAN. <i>The CIA in Guatemala: The Foreign Policy of Intervention.</i> By Robert Freeman Smith	781
		JOHN V. LOMBARDI. <i>Venezuela: The Search for Order, the Dream of Progress.</i> By Winfield J. Burggraaff	782
		NATHANIEL H. LEFF. <i>Underdevelopment and Development in Brazil. Vol. 1, Economic Structure and Change, 1822-1947; vol. 2, Reassessing the Obstacles to Economic Development.</i> By Warren Dean	782
		NOBLE DAVID COOK. <i>The People of the Colca Valley: A Population Study.</i> By Nicholas P. Cushner	783
Collected Essays	785	Communications	800
Documents and Bibliographies	793	Index of Advertisers	26(a)
Other Books Received	796		

The Angevin Strategy of Castle Building in the Reign of Fulk Nerra, 987-1040

BERNARD S. BACHRACH

COUNT FULK NERRA OF THE ANGEVINS—unlike his neighbors William the Great of Poitou and Aquitaine and Odo II of Blois, Chartres, Châteaudun, Tours, and Champagne—enjoyed an exceptionally successful reign. Historians have long recognized that his fortifications, a major aspect of his policy, contributed to this success. Indeed, in modern times he has come to be called “le grand bâtisseur.”¹ But scholars have yet to reach a consensus regarding how he used these fortifications or even why he elected to pursue a strategy of building that was not only costly but that had the potential to be used against him by recalcitrant castellans.²

Almost a century ago, Kate Norgate, whose outstanding ability to explicate the narrative sources for Fulk’s reign still commends her work, asserted that the Angevin count had both defensive and offensive purposes for his building operations. She boldly generalized that certain fortresses “served the double purpose of linking his outlying possessions in Touraine with his headquarters in Anjou, and of cutting in halves the dominions of his neighbour.” Other strong-

The research for this study was made possible by grants from the American Council for Learned Societies and the Graduate School of the University of Minnesota, and I am grateful for their generosity. In addition, I would like to thank Mlle. Poirier-Coutansais, director of the Archives de Maine-et-Loire, for her many kindnesses, and the staffs of the Archives de Maine-et-Loire, the Bibliothèque Municipale d’Angers, the Bibliothèque Nationale, and the Bibliothèque Municipale de Poitiers for their help. I am also indebted to Professor Steven Fanning of the University of Illinois, Chicago Circle, whose critical reading of this study saved me from making several careless errors.

¹ Louis Halphen, *Le Comté d’Anjou au XI^e siècle* (Paris, 1906), 153. For Fulk’s reign in comparison with those of his neighbors, see Bernard S. Bachrach, “A Study in Feudal Politics: Relations between Fulk Nerra and William the Great, 995-1030,” *Viator*, 7 (1976): 111-22, “Robert of Blois, Abbot of Saint-Florent de Samur and Saint-Mesmin de Micy, 985-1011” [*fidelis* of the counts of Blois], *Revue Bénédictine*, 88 (1978): 123-46, “The Idea of the Angevin Empire,” *Albion*, 10 (1978): 293-99, and “Toward a Reappraisal of William the Great, Duke of Aquitaine, 995-1030,” *Journal of Medieval History*, 4 (1979): 11-21.

² Scholars have long recognized the problems that were or could be caused by disloyal castellans. See, for example, P. Guilhaume, *Essai sur l’origine de la noblesse en France au moyen âge* (Paris, 1902), 143; Marc Bloch, *Feudal Society*, trans. L. A. Manyon, 2 (London, 1961): 400-01; and Sidney Painter, “Castellans of the Plain of Poitou in the Eleventh and Twelfth Centuries,” *Speculum*, 31 (1956): 243-57, and “English Castles in the Early Middle Ages: Their Number, Location, and Legal Position,” *ibid.*, 10 (1935): 321-31, which have been reprinted in [Painter] *Feudalism and Liberty: Articles and Addresses of Sidney Painter*, ed. Fred A. Cazell, Jr. (Baltimore, 1961), 17-40, 125-39. (All further references to Painter’s work will be to this volume.) Some lords were not unaware of these dangers. See, for example, the “Conventum inter Guillelmum Aquitanorum comes et Hugonem Chiliarum,” ed. Jane Martindale, *English Historical Review*, 84 (1969): 528-48. Both Martindale and George Beech have provided useful commentaries; see *ibid.*; and Beech, “A Feudal Document of Early Eleventh-Century Poitou,” in *Mélanges offertes à René Crozet*, 1 (Poitiers, 1966): 203-13.

holds, Norgate argued, posed “a standing menace” to enemy bastions, and yet others blocked the communication routes upon which his opponents depended to keep their own possessions secure.³

Norgate’s somewhat vague generalizations were rejected as insufficiently supported and superficial by Louis Halphen, the first scholar to exploit the documentary sources for Fulk’s reign in a systematic manner. Halphen focused upon the offensive aspects of Angevin strategy and argued that Fulk built the fortifications at Langeais, Montrichard, Montbazou, and Montboyau to sustain Angevin attacks in the Touraine. This type of offensive strategy, Halphen also suggested, lay behind building the stronghold at Trèves (ca. 1015), which prepared the way for Fulk to take Saumur, which fell in 1026. A defensive strategy, however, may have led to the strongholds at Montrevault, Montfaucon, and Saint-Florent-le-Vieil; Halphen argued that Fulk used these fortifications to secure Angevin domination of the Mauges region.⁴

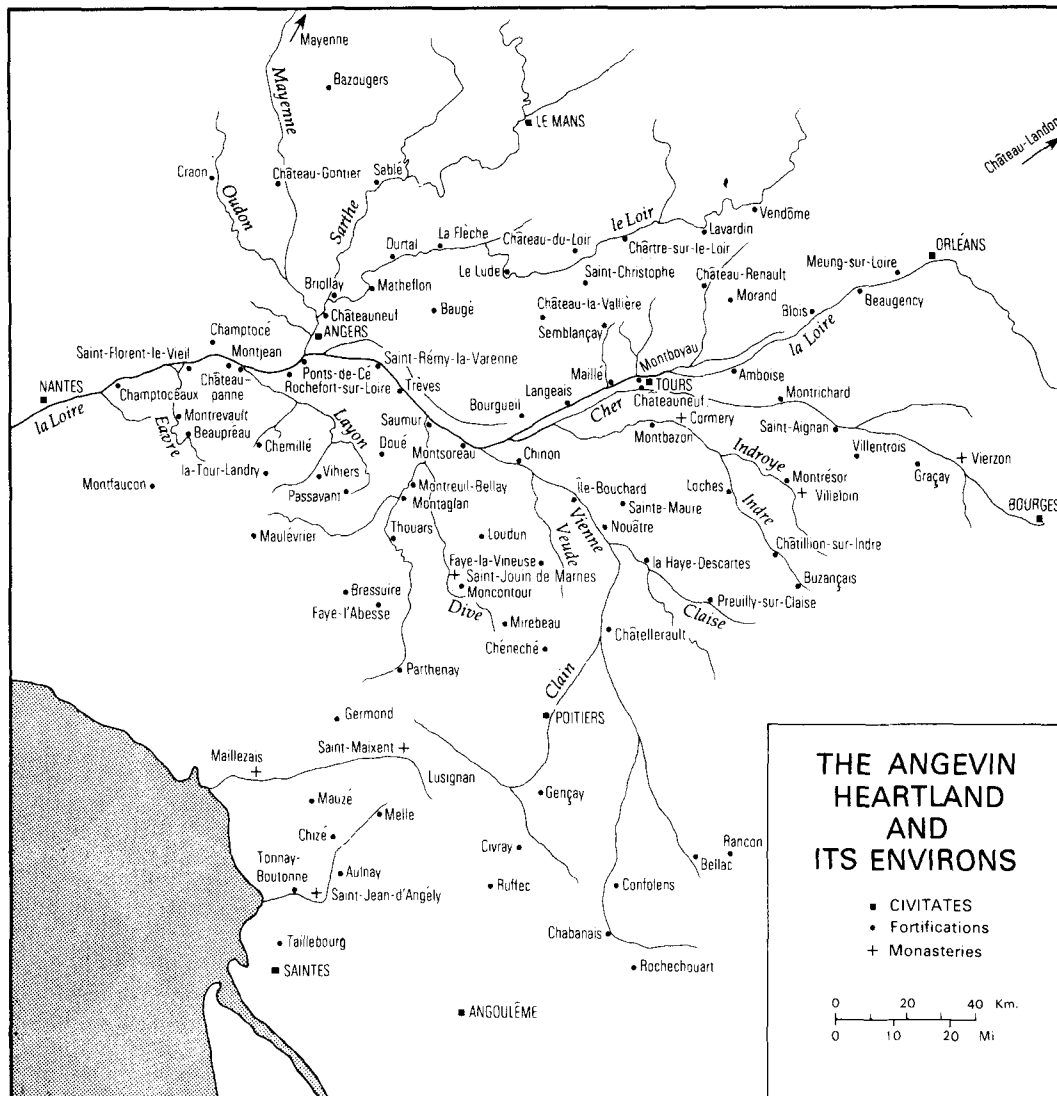
Unfortunately, Halphen discussed many Angevin strongholds—Mirebeau, Faye-la-Vineuse, Moncontour, Passavant, Maulévrier, Château-Gontier, Baugé, Matheflon, Durtal, Briollay, Beaupréau, Montjean, and Chemillé—without explaining why the Angevin count might have expended the resources in men and material necessary to these efforts, and Halphen never mentioned other fortifications that Fulk built with his own resources or with resources over which he had ultimate control. And Halphen’s assertion that Langeais was built for offensive purposes explicitly contradicts an Angevin tradition; that tradition did not take written form until the middle of the twelfth century, but it nevertheless needs to be given serious consideration.⁵ Finally, Saumur fell as the result of a surprise attack, and the stronghold of Trèves played no role in its subjection. Trèves was built primarily for the purpose of thwarting the westward advance of enemy troops, stationed at Saumur, through the Loire valley.⁶

³ Norgate, *England under the Angevin Kings*, 1 (London, 1887): 151–52. The first modern study of Fulk’s reign dates to the 1870s; the work is, however, seriously flawed, and as early as 1906 Halphen considered it to be “sans valeur.” See A. de Sables, *Histoire de Foulques-Nerra* (Angers, 1874); and Halphen, *Le Comté d’Anjou*, 11.

⁴ Halphen, *Le Comté d’Anjou*, 11, 26–27, 31–32, 37, 52, 153–57.

⁵ I will discuss the fortifications Halphen did not consider and note those Halphen ignored both in his book and his earlier article, “Étude sur l’authenticité du fragment chronique attribué à Foulque le Réchin,” *Bibliothèque de la Faculté des Lettres de Paris*, 13 (1901): 7–48. For the Langeais tradition, see my “Fortifications and Military Tactics: Fulk Nerra’s Strongholds circa 1000,” *Technology and Culture*, 20 (1979): 534 n. 12.

⁶ Halphen provided a satisfactory discussion of the fall of Saumur; *Le Comté d’Anjou*, 39–41. Although I disagree on certain minor points, his account makes clear that Trèves played no role in the campaign. My conclusions concerning the chronology of and purpose in building Trèves have been reconstructed from a variety of sources that form, when treated in ensemble, a useful and significant body of circumstantial evidence. First, sometime after 1014 Fulk undertook to provide protection for lands belonging to Saint-Aubin that were located in la Vallée—that is, in the region of the Loire valley eastward from Ponts-de-Cé to the *curtis* of Saint-Rémy-la-Varenne. Indeed, Saint-Aubin obtained lands in this area only in 1014; B. de Broussillon, ed., *Cartulaire de Saint-Aubin-d’Angers* (Angers, 1896), no. 178. For Fulk’s agreement with Saint-Aubin to provide protection, see the notice included in an act that was drawn up on May 29, 1029; *ibid.*, no. 932. This notice makes clear that the monks of Saint-Aubin sought Fulk’s protection because they had suffered severely in this region from enemy raiders. Sometime following this agreement with the monks, Fulk launched an attack on the Saumurois, which resulted in the building of the stronghold at Trèves; *Historia Sancti Florentii Salmurensis*, in P. Marchegay and E. Mabille, eds., *Chroniques des églises d’Anjou* (Paris, 1869), 275–76. The author of this *Historia* followed up his account of the building of Trèves with a discussion of Fulk’s construction of the stronghold at Montboyau, definitely assignable to 1017; Halphen, *Le Comté d’Anjou*, 37. And Fulk is known to have spent the spring and summer of 1016 in the campaign that culminated in the battle of Pontevoy; *ibid.*, 34–36. Therefore,



Halphen's insistence on the offensive nature of Fulk's building operations in the Touraine seems to have led Sir Richard Southern to generalize even more boldly: "The town of Tours, for instance, was not swallowed until 1044 [by Fulk's son], but in a sense the whole history of the family [of the Angevin counts] was a preparation for this event, . . . the encircling of the town by a ring of castles at Langeais, Montbazou, Montrichard and Montboyau had been begun by Fulk Nerra fifty years before the final victory." Yet Southern does appear to have been worried about the role historical hindsight might have played in this reconstruction of Fulk's plans, because they took a half-century to mature. Southern therefore covered his flank by observing, "How much was design and how much a kind of inspired

the campaign that led to the construction of Trèves must have occurred in 1015. The chronology should look something like this: early in 1014 Saint-Aubin acquired lands in la Vallée, which were raided that spring by Gelduin. At the monks' request, Fulk agreed to provide protection and, in late 1014 or early 1015, launched the campaign that resulted in building the stronghold at Trèves. This blocked enemy movement westward into the valley of the Loire, where Saint-Aubin's lands were located.

opportunism it would be useless to enquire. Once started, the process went on as relentlessly as the operation of the Stock Exchange.”⁷

Southern, always the consummate stylist, seems to have had in mind the operation of some sort of subconscious manifest destiny that governed Fulk’s strategic decision-making processes; in lieu of the equivalent of a “Hossbach memorandum,” at any rate, he appears to have been unwilling to give the Angevin count and his son, Geoffrey Martel, credit for deliberate policy of more than short duration. By the time that Fulk Nerra had succeeded his father, Geoffrey Greymantle, however, the Angevin counts had already demonstrated their ability to lay plans that took many years to be realized. Perhaps the most notable example of their foresight and planning is evidenced in the *conventiae* between Geoffrey Greymantle and Renaud of Thorigné, viscount of Angers.⁸ Southern has, therefore, underestimated Geoffrey and his descendants.

Although Southern followed Halphen’s scheme and concentrated on Fulk’s strategy in the Touraine, Oliver Guillot ventured a broadly based generalization and suggested that the great builder’s *châteaux* were “le moyen plus important de soutenir son pouvoir.”⁹ Thus Guillot implied that Fulk built these strongholds as a means of sustaining comital power. Some support for drawing such an inference

⁷ Southern, *The Making of the Middle Ages* (New Haven, 1953), 88. Southern’s appreciation of the strategic situation was impeded by his failure to control for several important details. First, Langeais was clearly built for defensive purposes—to secure Fulk’s lines of communication; see note 15, below. In addition, although Langeais is only about thirty kilometers down river from Tours, Fulk’s more obvious strategic base against Tours was Maillé. Southern appears to have been unaware, moreover, of the stronghold at Maillé or of Fulk’s agreement in 1027 to destroy Montboyau as part of a treaty that recognized his *de jure* possession of Saumur; *Historia Sancti Florentii*, 280. Finally, Southern failed to consider two other strongholds—Morand and Semblançay—as part of Fulk’s strategy for encircling Tours. In short, Fulk’s investment of men and resources both directly and indirectly for the encirclement of Tours was much greater than Southern recognized. Although such additional information surely strengthens Southern’s point about the importance of Tours to the Angevins, it also seems to suggest a greater degree of design on Fulk’s part than is evidenced by the smaller number of strongholds to which Southern referred.

⁸ Sometime before the death of Bishop Nefingus on September 12, 973, Geoffrey Greymantle made several *conventiae* with Renaud of Thorigné, viscount of Angers, to the effect that the viscount’s elder son, also named Renaud, would replace Nefingus when the bishop died. In return for this promise, the viscount agreed that the family’s alods in the Mauges region, which otherwise would have been part of the younger son’s inheritance to pass on to his own offspring, would pass into the Angevin comital fisc on the death of the future bishop, the younger Renaud. The ages of those affected by the agreement—or, perhaps more accurately, their life expectancies—make it clear that Geoffrey could not have reasonably expected the *conventiae* to have directly benefited the comital fisc during his own lifetime. Geoffrey, who was born no later than ca. 940, died in 987; Viscount Renaud died in the early 990s; and his elder son died in 1005, after serving as bishop for thirty-two years. For the *conventiae* between Geoffrey and the elder Renaud, see C. Urseau, ed., *Cartulaire noir de la Cathédrale d’Angers* (Angers, 1908), no. 25. On the Renaud clan, see Richard M. Hogan, “The Ramaldi of Angers: ‘New Men’ or Descendants of Carolingian Nobles?” *Medieval Prosopography*, 2, no. 1 (1981): 35–62. An equally clear example of Angevin foresight is Geoffrey’s plan for Angevin acquisition of the *comté* of Vendôme. In the mid-980s Geoffrey arranged the marriage of his son Fulk Nerra to Elizabeth of Vendôme, the almost certain heiress of the county (her father the count was an aged widower, unlikely to sire any more children, and her brother was a cleric in the royal entourage, unlikely to father any legitimate offspring). A somewhat more unusual example of such planning for the future is Geoffrey’s association of his son in the comital title; Geoffrey was the first great magnate in the west of France to take such a step. On both the marriage and the association, see Bernard S. Bachrach, “Fulk Nerra and His Accession as Count of Anjou,” in M. King and W. Stevens, eds., *Saints, Scholars, and Heroes: Studies in Medieval Culture in Honor of Charles W. Jones*, 2 (Collegeville, Minn., 1979): 335–36, and, more generally, “The Idea of the Angevin Empire,” *Albion*, 10 (1978): 293–99.

⁹ Guillot, *Le Comte d’Anjou et son entourage au XI^e siècle*, 2 vols. (Paris, 1972), 1: 281. Halphen did not hesitate to hazard a generalization of no less crucial insight concerning Fulk’s strongholds, observing that they provided refuge for the people of the countryside during enemy raids; *Le Comté d’Anjou*, 84.

derives from the restraint that Geoffrey Martel, Fulk's son and successor, showed in building fortifications; he apparently understood that his castellans could usurp control of these strongholds and thus weaken comital power. Indeed, Guillot evinced little direct interest in Fulk's military strategy but instead directed his attention to what may be termed the count's constitutional-military strategy—that is, the role played by these strongholds in the decline of comital power (the disintegration of the *pagus*). Within this framework, Guillot identified several strongholds as “châteaux en marche”: Montsoreau, Craon, Trèves, Mayenne, Briollay, Faye-la-Vineuse, Champtoceaux, Montjean, and la Chartre-sur-le-Loir. These were, he suggested, precocious seigneuries insofar as they escaped direct comital control earlier than other strongholds.¹⁰ But little evidence supports Guillot's assertion that these strongholds either when built or captured by Fulk Nerra soon became *seigneuries châtelaines*. When a particular castellan did enjoy seigneurial rights, moreover, the fortification in question was no longer *en marche*.¹¹ And, although many of Fulk's other strongholds originally were built *en marche*, there is no evidence to suggest that they were precocious seigneuries.¹²

Most recently, Marcel Deyres, an archaeologist specializing in the military architecture of the Touraine, has shown little patience with previous characterizations of Fulk's military aims and has argued that the Angevin count's purpose in building *châteaux* was tactical rather than strategic. According to Deyres, Fulk built meager wooden strongholds, not the formidable stone fortifications that won for him the sobriquet “le grand bâtisseur,” so that he might pursue a “guerre de vif mouvement.” In short, these wooden fortifications purportedly served as bases for an early version of *Blitzkrieg* tactics carried out by Fulk's cavalry. To sustain this thesis, Deyres dealt only with a very small selection of Fulk's strongholds (primarily Langeais and Montbazou but also Château-Gontier, Saint-Florent-le-Vieil, Mont-

¹⁰ Guillot, *Le Comte d'Anjou et son entourage*, 1: 310–11, 315.

¹¹ Trèves, for example, was constructed in 1015, but the earliest date for which Guillot could argue for a seigneurial regime there is 1068; *Le Comte d'Anjou et son entourage*, 1: 343; and note 6, above. Nor could he show that Mayenne, which Fulk captured in 1014, became a seigneurial stronghold following Angevin possession; *Le Comte d'Anjou et son entourage*, 1: 311, 457–58. The same may be said for Montjean and certainly for Champtoceaux, which was in the hands of Renaud of Thorigné by 988 but is first noted as having a *dominus* after 1040; *ibid.*, 458–60. Much the same problem obtains for Guillot's other examples. In each instance, we need to prove—not assume—that the phrase *dominus castri* refers to a particular castellan with seigneurial rights to a specific fortification, define clearly what seigneurial rights were involved, and ascertain if the documents demonstrate that such rights were held by the *dominus*. Guillot has not carried out this type of analysis, and my research suggests that the evidence for such broad generalizations does not exist. Moreover, the time sequences in the evidence available actually indicate that seigneurial privileges substantially postdate frontier status. Trèves ceased to be a frontier stronghold after the fall of Saumur in 1026, but the castellan did not have “seigneurial rights” there before 1068; *ibid.* Montsoreau also ceased to be a frontier stronghold after the fall of Saumur or, at the latest, after the fall of Chinon (ca. 1037–38); for the date, see note 93, below. Although Guillot argued that the phrase *castello suo* demonstrates that Montsoreau was a *seigneurie châtelaine* by 1001, Fulk's power over that stronghold and its castellan makes its frontier independence unlikely; compare *ibid.*, and Bibliothèque Nationale, Paris [hereafter, BN], Coll. D. Houss. 11.1, no. 326. Also see Bachrach, “Robert of Blois, Abbot,” 136–37. For discussions of Langeais, Mirebeau, Vihiers, Montfaucon, Montreuil-Bellay, and Montrichard, see below.

¹² Whether or not a particular castellan had seigneurial rights within and around the stronghold for which he was responsible is, however, of little direct military importance. Militarily speaking, the vital question is whether or not the castellan served the count loyally and to the best of his ability when the need arose. See John Le Patourel, *The Norman Empire* (Oxford, 1976), 305.

boyau, and Montreuil-Bellay) and endeavored to demonstrate that these were built of wood rather than of stone.¹³ Deyres's arguments in two crucial cases, Langeais and Montbazou, are seriously flawed, based on erroneous methods for handling the available evidence. More to the point, however, is that, in those very few cases where a plausible (though not conclusive) case can be made for the use of wood in even part of the construction of a particular fortification, Deyres has failed to show that Fulk then engaged in a "guerre de vif mouvement" after 992, when the count first began to construct strongholds.¹⁴ To assert that a fortification was not built of stone is hardly sufficient to prove that Fulk Nerra employed *Blitzkrieg* tactics from hastily built bases constructed of wood. Thus far, then, scholars have failed to appreciate and, therefore, to provide an integrated analysis of Fulk's building activities within the overall military policy of his reign.

DURING THE FIRST DECADE OF HIS REIGN, Fulk's concerns were primarily defensive. His western frontier was insecure, and his holdings in the Touraine were endangered. Blésois control of the Saumurois threatened to cut off Angevin communications with the Touraine, and Abbot Robert of Saint Florent, who was one of Count Odo of Blois's two most important supporters in the region (the other was Gelduin of Saumur), schemed to deprive Fulk of logistic support by undermining the loyalty of Viscount Renaud to his lord and count. The *Historia Sancti Florentii* notes in characteristic hyperbolic discourse Gelduin's efforts to block Fulk's access to the east: "How many times Fulk, count of the Angevins, as he was crossing through this neighborhood [the Saumurois] would say in terror, 'let us get away from the devil of Saumur; I always seem to see him in front of me.'" Indeed, Fulk Nerra's first important building operations, which were carried out at Langeais between the summer of 992 and early 994, were intended, according to this same local chronicler, to provide the Angevin count with secure bases to assure communications with his strongholds at Amboise, Loches, Villentrois, and la Haye-Descartes.¹⁵

Fulk found it necessary to undertake what became a lengthy and extensive effort to build numerous strongholds to establish secure communications with his eastern holdings because Odo I of Blois had abandoned traditional Blésois policy of cooperation and posed a serious threat to Angevin possessions in Touraine and

¹³ Deyres, "Les Châteaux de Foulque Nerra," *Bulletin Monumental*, 132 (1974), 9–10, "Le Donjon de Langeais," *ibid.*, 128 (1970): 179–93, and "Le Château de Montbazou au XI^e siècle," *Cahiers de civilisation médiévale*, 12 (1969): 147–59.

¹⁴ It is perhaps ironic that in the period before Fulk began to build strongholds—that is, before his victory at the battle of Conquereuil in 992—the Angevin count did pursue a strategy of hit-and-run raids to keep his enemies off balance, as the chronicler Richer, Fulk's contemporary, duly noted; Richer, *Histoire de France, 888–995*, ed. R. Latouche (Paris, 1930), Bk. IV, chaps. 74–75. After his victory at Conquereuil, however, Fulk abandoned any type of strategy that might be considered a "guerre de vif mouvement." For Langeais and Montbazou, see Bachrach, "Fortifications and Military Tactics," 531–49.

¹⁵ *Historia Sancti Florentii*, 275. It is perhaps surprising that monkish chroniclers were aware of such matters of logistical detail. See, in addition, *Gesta Ambaziensium Dominorum*, in Louis Halphen and R. Poupardin, eds., *Chroniques des comtes d'Anjou et des seigneurs d'Amboise* (Paris, 1913), 90; and *Chronica de gestis consulum Andegavorum*, *ibid.*, 48. Taken together, these examples surely suggest that at least some monkish chroniclers were well aware of what secular leaders thought was of military importance. For the general situation and Abbot Robert's activities in the 990s, see Bachrach, "Robert of Blois, Abbot," 126–33, and "Fortifications and Military Tactics," 547–49.

northeastern Berry. Geoffrey Greymantle, Fulk's father, had taken an important step in preparing to deal with Odo's threat by establishing an alliance with Count Bouchard of Vendôme. This alliance was of considerable strategic importance because the Vendômois separated Blois from Count Odo's northern lordships at Châteaudun and Chartres.¹⁶ In addition, the stronghold at Vendôme itself commanded the middle reaches of the Loir valley, and Bouchard's supporters were influential in the northern Touraine.¹⁷ Finally, Vendôme was only forty-five kilometers north of Amboise and provided a base from which a relief expedition could be dispatched to Fulk's holdings to the south.

The Vendôme alliance was, however, only the preliminary stage of a longer-term policy. Fulk had to be in a position to relieve not only the fortification at Amboise but also those at Villentrois, Loches, Châtillon-sur-Indre, Buzançais, Nouâtre, la Haye-Descartes, and Preuilly-sur-Claise.¹⁸ Unless Fulk could establish secure lines of communication with these strongholds, the castellans, lords, and garrisons could not be expected to hold out against any efforts Odo's men launched against them. Knowledge that lines of communication were secure so that relief forces might be sent was vital to the morale of the men who garrisoned such fortifications. Indeed, the custom appears to have been to spare the lives of those defenders who surrendered (*sub lege deditionis*) before hostilities commenced in earnest but to put to the sword those members of the garrison who held out and were captured when the stronghold fell.¹⁹ Therefore, unless relief could be expected, the garrisons of such fortifications had little incentive to stand their ground and every reason to surrender.

In addition to providing the necessary incentive to his castellans, Fulk had to instill confidence in his *fideles*—the lords, for example, of Buzançais, Nouâtre, and Preuilly-sur-Claise—that he could protect their strongholds and lands against the aggression and depredations of the count of Blois. Indeed, if Fulk could not maintain his own strongholds and support his *fideles*, his supporters might have had to transfer their loyalty to the count of Blois to avoid losing their lands. Fulk's position had already been seriously weakened in the early 990s in just such a

¹⁶ For the background, see Karl F. Werner, "L'Acquisition par la Maison de Blois des comtés de Chartres et de Châteaudun," *Mélanges de numismatique, d'archéologie, et d'histoire offerts à Jean Lafaurie* (Paris, 1980), 265–72. Also see Bachrach, "Fulk Nerra and His Accession," 336, and "Robert of Blois, Abbot," 126–32, 134–37.

¹⁷ For useful material on several of these interconnected families, see Steven C. Fanning, "Les Origines familiales de Vulgrin, abbé de Saint-Serge de Angers (1046–1056) et Evêque du Mans (1056–1065), Petit-fils du vicomte Fulcrade de Vendôme," *La Province du Maine*, 82 (1980): 245–50.

¹⁸ For Fulk's role in relation to Loches, la Haye-Descartes, and Villentrois, see Halphen, *Le Comté d'Anjou*, 4; concerning Preuilly-sur-Claise, see BN, Coll. D. Houss. II.1, no. 166; and L. Delisle, ed., *Recueil des historiens des Gaules et de la France*, 10 (Paris, 1874): 600; concerning Nouâtre, see A. Salmon, ed., *Livre des serfs de Marmoutier* (Paris, 1845), no. 1; and, concerning Châtillon-sur-Indre and Buzançais, see Guy Devaillay, *Le Berry du X^e siècle au milieu du XIII^e siècle* (Paris, 1973), 167. Aelfredus of Preuilly-sur-Claise was one of Fulk's men; *Recueil des historiens des Gaules et de la France*, 600. Geoffrey Greymantle spent some time at the *castrum* of Nouâtre in the summer of 985 and issued a charter while he was there; one of the *boni homines* who supported the count's act is styled "Gemmo," and he should probably be identified with "Guenno," the *dominus* of this stronghold who was one of the Angevin count's *homines*; see *Livre des serfs de Marmoutier*, no. 1; and *Chronica de gestis consulum Andegavorum*, 48.

¹⁹ It is certainly noteworthy that clerics paid attention to such technicalities when writing their chronicles. Richer even recognized that garrison troops needed to have an active hope of relief to sustain their defense; *Histoire de France*, Bk. 4, chap. 86. Also see *Chronica de gestis consulum Andegavorum*, 47–48; and *Historia Sancti Florentii*, 277. For the *lex deditionis*, see M. H. Keen, *The Laws of War in the Late Middle Ages* (London, 1965), 128.

situation. Fulk transferred Château-Landon to his cousin Walter when he could not extend his own resources so far to the east.²⁰ Too many such losses on the periphery—without strongholds and *fideles* to support him—would very probably have undermined the loyalty of his *homines* in the Touraine, including control of the monasteries of Cormery and Villeloin, whose fisco constituted valuable Angevin assets.²¹

Fulk's prospects of defending his possessions in Touraine brightened considerably in March 996, when Odo I suddenly took ill and died. The Angevin count took advantage of this turn in the wheel of fortune, seized Odo's stronghold at Montsoreau, and, in alliance with his cousin-in-law, Count Aldebert of la Marche and Périgord, captured Tours and its fortified suburb of Châteauneuf.²² This attack on Tours seems to indicate that Fulk had decided to develop a direct line of communication with his stronghold at Amboise. Before the end of 996, Fulk controlled the Loire valley from Montsoreau in the west to Amboise in the east. From his base at Langeais, Fulk also had access to the valley of the Indre, his stronghold at Loches, and the lands of his *fideles* further to the south. Only Blésois control of the Saumurois kept Fulk from having fully secure lines of communication with his eastern holdings in Touraine and northeastern Berry.

It seems unlikely, however, that Fulk's decision to construct a stronghold at Langeais ca. 992 represents the first step in a master plan for a direct assault on Tours. The campaign in 996 seems rather to have resulted from a combination of fortuitous circumstances, particularly Odo's death, which left the Blésois in disarray, and Aldebert's victory over William the Great's forces, which nullified the prospect of Poitevin intervention. The unexpectedness with which the opportunity to take Tours appeared was matched by the surprising shift in allegiances that followed. Hugh Capet died, Aldebert was killed, and King Robert married Bertha of Blois and then recaptured Tours. Almost overnight, with Robert's marriage into the house of Blois and presence in Tours, the Capetians not only were no longer Angevin allies but had also become a threat to Fulk's position. Without Hugh Capet's support, Fulk was forced to develop new plans for securing his lands and lordships in the Touraine.²³

Prior to the capture of Tours, Fulk had gained a measure of success by avoiding

²⁰ For Château-Landon, which came to the Angevins in the ninth century, see Karl F. Werner, "Untersuchungen zur Frühzeit des französischen Fürstentums," *Die Welt als Geschichte*, 18 (1958): 272. Fulk's cousin Walter was of the family of the counts of Vexin, Valois, and Amiens; Philip Grierson, "L'Origine des comtes d'Amiens, Valois, et Vexin," *Le Moyen Âge*, 49 (1939): 96–97. Walter was in possession of this stronghold and was made count of Gâtinais before the battle of Orsay, which took place in 993 at the latest; Ferdinand Lot, *Les Derniers Carolingiens: Lothair, Louis V, Charles de Lorraine, 954–991* (reprint edn., Paris, 1981), 170; and B. Guérard, ed., *Cartulaire de l'Église Notre-Dame de Paris* (Paris, 1850), no. 18. By 997, however, Walter was helping Fulk against the Angevin's enemies; Abbo, *Epist.*, I, ed. J. P. Migne, *Patrologia Latina* (Paris, 1880), 139.

²¹ For Fulk's rights over these monasteries and their fisco, see Guillot, *Le Comte d'Anjou et son entourage*, 1: 163–71. Guillot has, however, tended to overestimate the importance of "reform" in limiting the ability of the count to use the resources of these houses as he wished. "Reform" may have weakened the count's legal position, but, as a charter reproduced in the *Recueil des historiens des Gaules et de la France* makes clear, Fulk did what he wanted when military exigency was the issue; *Recueil des historiens des Gaules et de la France*, 577.

²² Bachrach, "A Study in Feudal Politics," 114–15, and "Robert of Blois, Abbot," 137 n. 2.

²³ Bachrach, "A Study in Feudal Politics," 114–15, "Robert of Blois, Abbot," 131–32, and "Fortifications and Military Tactics," 531–49.

battle when possible and building strongholds that did not pose an overt challenge to an enemy position. Fulk took his indirect strategy so seriously that the first stronghold that he built, albeit a minor one to the east of Bourgueil, was constructed in secret. When Fulk shifted to a direct strategy and captured Tours, he soon learned what he apparently suspected before, that the overt approach galvanized enemy opposition. Probably in reaction to this, Fulk returned to an indirect strategy and abandoned for almost twenty years any effort to pose a direct challenge to Tours and the royal presence there.²⁴

Fulk tried to avoid the communications problems that resulted from an enemy-held Tours by fortifying a secure route between Amboise and Angers that ran through the northern Touraine. Seventeen kilometers north of Amboise at Morand, Fulk fortified a *domus* on one of his estates, and his supporters knew provisions were to be found there. From Morand, Fulk's route went west-southwest thirty-three kilometers to Semblançay, where the Angevin count also built a stronghold.²⁵ The next stop on this route was at Château-la-Vallière, twenty-three kilometers to the west. This stronghold belonged to Fulk's *homo*, Hugh of Alluyes, who also held the fortification of Saint-Christophe. The remainder of the route continued west thirty-two kilometers to the stronghold that Fulk constructed at Baugé.²⁶ From Baugé it was only another thirty-three kilometers to Angers.

This fortified line, which secured Angevin communications, took some time to construct, and it can hardly be construed as an accident that each of the distances between strongholds on this route from Angers to Amboise could be negotiated by a mounted force in less than a day's ride with mounts that were still in condition to sustain their riders in combat. If the distances between fortifications were much in excess of thirty-five kilometers, then any force moving along the route would have neared its destination with mounts unable to engage in sustained combat; if the distances were in excess of fifty kilometers, the horses would not have been able to continue the journey or fight on the following day, making camping in the open for the night necessary. Neither of these alternatives could have been acceptable. For a relief force to cover ground quickly, moreover, it could not be encumbered with the

²⁴ *Historia Sancti Florentii*, 274; and *Gesta Ambaziensium Dominorum*, 82.

²⁵ For Semblançay, see *Chronica de gestis consulum Andegavorum*, 48; and, for Morand, see *ibid.*; and *Fondation de Châteaurenault*, in *Chroniques des comtes d'Anjou*, 149. From the chronology found in the *Chronica de gestis consulum Andegavorum*, the author of this part of the text evidently believed that Morand and Semblançay were built before the defection of Landry of Dun. Landry's defection occurred during Fulk Nerra's second pilgrimage to the Holy Land, so these fortifications were constructed prior to ca. 1010—roughly within the same time frame as Montbazou and Montrichard. For the chronology of Montbazou, see Bachrach, "Fortifications and Military Tactics," 547–49; and, for that of Montrichard, see Halphen, *Le Comté d'Anjou*, 31–32, 153. Had Landry defected much earlier than 1010, Fulk most likely would have crushed the rebel prior to leaving on his pilgrimage. Not until ca. 1014, moreover, did the Angevin count replace Landry with Lisois of Bazougers at Amboise: see *Gesta Ambaziensium Dominorum*, 48, 54–56; and *Chronica de gestis consulum Andegavorum*, 76, 79. Halphen, however, dated Landry's defection to ca. 987, shortly after Fulk came to power; *Le Comté d'Anjou*, 15–17. Halphen's date is undoubtedly in error, not only because of Landry's treatment in the *Chronica* but also because of the appearance of a prominent *fidelis* named Landry in Fulk's entourage in 999, more than a decade after—according to Halphen—he disappeared; *Chronica de gestis consulum Andegavorum*, 76–79; and BN, MS Lat. 17,127, pp. 157–59. Landry of Dun was, moreover, the only Landry in Fulk's entourage before 1026.

²⁶ For Château-la-Vallière, see *Chronica de gestis consulum Andegavorum*, 48; and, for Baugé, see Halphen, *Le Comté d'Anjou*, 156 n. 1.

impedimenta necessary to establish a fortified encampment; ox carts could make no more than fifteen kilometers per day, and the Angevins did not use horses for draught purposes.²⁷ Fulk and his *fideles* cannot have been uninformed concerning the particular advantages inherent in a well-defended route.²⁸

Although the placement of these strongholds surely was the result of such strategic considerations, it would be hasty to conclude that all of these fortifications were sited for the sole purpose of establishing a secure route between Angers and Amboise. Fulk chose to build a stronghold at Morand, for example, seventeen kilometers north of Amboise. If Fulk had wanted only to provide a secure position north of Amboise and on the direct route to Semblançay, he probably would not have built at Morand but somewhere roughly eight kilometers further south. His choice was quite likely influenced by the location of Vendôme, only twenty-eight kilometers north of Morand. Thus the fortification at Morand helped secure this north-south route between Amboise and Vendôme as well as provide a fortified step in his east-west link between Amboise and Angers.

²⁷ Mounted travelers during the earlier Middle Ages could generally cover from thirty to just under forty kilometers per day, and a small mounted column without remounts could be expected to do as well; Pierre Riché, *Daily Life in the World of Charlemagne*, trans. J. McNamara (Philadelphia, 1978), 22–23. But, because it might be called on to fight at any moment, a military force could not afford to place itself in the position of tiring its mounts while moving through hostile or potentially hostile territory, if it intended to be ready at all times to be able to fight on horseback and not to be forced to dismount and fight on foot because of tired mounts. “The question is not whether speeds in excess of twenty or twenty-five miles a day were attainable,” as J. W. Nesbitt has pointed out, for “these speeds were made by either a single rider or small party of riders who in both cases were covering a limited number of miles and for a short period of time.” He observed that crusading armies—depending on the terrain, road conditions, and supply arrangements—averaged as few as fifteen kilometers and as many as twenty-nine kilometers per day. Nesbitt, “The Rate of March of Crusading Armies in Europe: A Study and Computation,” *Traditio*, 19 (1963): 174. And draught animals reduced the rate an entirely mounted force could travel; Donald W. Engels, *Alexander the Great and the Logistics of the Macedonian Army* (Berkeley and Los Angeles, 1978), 15; and Albert Leighton, *Transport and Communications in Early Medieval Europe, A.D. 500-1000* (Devon, 1970), 159. Although the availability of remounts would have extended the distance a mounted force could cover in a day, because a rider is far more durable than his horse, there is no evidence that Fulk’s forces had remounts, and the problems involved in feeding remounts—among other things—made them a less than fully satisfactory alternative to keeping the maximum distance between fortifications at about thirty-five kilometers; Engels, *Logistics of the Macedonian Army*, 129–30; and R. P. Lindner, “Nomadism, Horses, and Huns,” *Past & Present*, no. 92 (1981): 15. Vegetius, a mid-fifth-century military commentator and veterinarian who knew a great deal about horses, recommended that cavalry forces take practice rides that totaled twenty Roman miles (thirty-two kilometers; the Roman mile was approximately 1,618 English yards) to condition themselves and their mounts for more lengthy journeys; Vegetius, *Flavii Vegetii Renati Epitoma Rei Militaris*, ed. C. Lang (Leipzig, 1885). (Since horses were traditionally trained at under distances, a five kilometer addition may be made to Vegetius’s training figures.) Of course, any successful military commander of the eleventh century did not have to read Vegetius to learn that horses easily tire and would be of little use in combat if they are pushed very much beyond thirty-five kilometers, but Fulk was probably familiar with Vegetius’s works; see Bernard S. Bachrach, “The Practical Use of Vegetius’ *De Re Militari* during the Early Middle Ages,” *Historian* (forthcoming). Also see Lindner, “Nomadism, Horses, and Huns,” 12. For Angevin use of oxen, but not horses, for draught purposes, see Jacques Boussard, “La Vie en Anjou aux XI^e et XII^e siècles,” *Le Moyen Âge*, 56 (1950): 55, 61; and J. M. Bienvenu, “Recherches sur les péages angevins aux XI^e–XIII^e siècles,” *ibid.*, 63 (1957): 209–40, 437–67. And a charter mentions the special facilities at Angers for Fulk Nerra’s ox herders or oxen; Y. Maiffert, “Fondation du monastère bénédictin de Saint-Nicholas d’Angers,” *Bibliothèque de l’École des Chartes*, 92 (1931): p.j. no. 4.

²⁸ Vegetius, whose maxims were widely circulated during the Middle Ages and even during the earlier Middle Ages, provided several crucial observations concerning the importance of a fortified encampment and the desirability of avoiding combat with tired troops. He also noted the defensive significance of closely linked strongholds. And I am inclined to believe that Fulk knew Vegetius’s works; Bachrach, “The Practical Use of Vegetius’ *De Re Militari*.” For the best catalogue of medieval Vegetian materials, see C. R. Shrader, “A Handlist of Extant Manuscripts containing the *De Re Militari* of Flavius Vegetius Renatus,” *Scriptorium*, 33 (1979): 280–305. Shrader’s “Handlist” has been supplemented by R. McKitterick’s “Charles the Bald, 823–877, and His Library: The Patronage of Learning,” *English Historical Review*, 95 (1980): 31–32.

When choosing to fortify Morand, Fulk also may have taken into consideration the short distance—less than a half-day's travel—between this new stronghold and Amboise. Defensively, Morand could serve as a base from which any enemy siege emplacements around Amboise could be harassed, and, similarly, any effort to besiege Morand could be relieved from Amboise. Offensively, Morand is positioned so that forces established there could harass communications and supply lines between the enemy strongholds of Blois and Château-Renault. Indeed, Morand was sited only nine kilometers east of the Blésois stronghold at Château-Renault. Fulk may well have considered that Morand—in addition to being part of the route from Amboise to Angers to the west, part of the route from Amboise to Vendôme to the north, and a potential threat to Blésois communications—might be ideal as a base camp for a siege of Château-Renault. Troops and supplies could easily be moved north from Amboise and, with slightly greater difficulty, south from Vendôme to Morand, and the stronghold could serve as a shelter for a prepositioned reserve to cut off any force sent from Blois to relieve Château-Renault.²⁹

While the northern route to Amboise from both Vendôme and Angers helped Fulk defend Angevin holdings in the northern Touraine, these strongholds did nothing to secure Fulk's lines of communication and lands further to the south and east. The Angevins dominated the middle reaches of the Indre valley—with fortifications at Buzançais, Châtillon-sur-Indre, and Loches—and the middle stretch of the Claise-Vienne chain—with strongholds at Preuilly-sur-Claise, la Haye-Descartes, and Nouâtre. Fulk's route from Angers to the east first went thirty-eight kilometers south to the stronghold at Vihiers and then turned east-southeast fifty kilometers to Loudun. From Loudun the route continued thirty-seven kilometers to Nouâtre, and from there it was only thirty-six kilometers to Loches, which was located thirty-one kilometers south of Amboise.

The fortified western links of this route had been forged rather hastily by Geoffrey Grey mantle in the wake of Odo I's change of policy toward the Angevins.³⁰ Geoffrey established his relative Alberic at Vihiers in the early 980s, Guenno of Nouâtre first appears in the Angevin entourage in the summer of 985, and the tower at Loudun had been built earlier for different purposes.³¹ The

²⁹ For Blésois control of Château-Renault, see Halphen, *Le Comté d'Anjou*, 48. Many historians have suggested that the use of reserves, especially prepositioned or strategic reserves, was beyond the capacity of medieval commanders. Such reserves were, however, used even during the early Middle Ages; John H. Beeler, *Warfare in Feudal Europe, 730-1000* (Ithaca, N.Y., 1971), 23. And Fulk Nerra certainly was familiar with the use of a tactical reserve, for he deployed just such a reserve at the battle of Pontevoy in 1016; Halphen, *Le Comté d'Anjou*, 34–36.

³⁰ Evidence for close relations between the Angevins and the Blésois during the lifetime of Theobald of Blois ("the Trickster") is the marriage of Fulk the Good (d. 960) to Theobald's sister, and Geoffrey Grey mantle, Fulk's son, later married Theobald's sister-in-law; Werner, "Untersuchungen zur Frühzeit des französischen Fürstentums," 266–69; and Guillot, *Le Comte d'Anjou et son entourage*, 1: 8–10. Also see R. Merlet, ed., *Chronique de Nantes* (Paris, 1896), chap. 37; Michel Bur, *La Formation du comté de Champagne, v. 950–v. 1150* (Nancy, 1977), 513; and BN, Coll. D. Houss. II.1, no. 181. For the gradual change in policy carried out by Odo I, see *Cartulaire de Saint-Aubin*, no. 3. By 987, Geoffrey and Odo I were overtly hostile to each other; Bachrach, "Robert of Blois, Abbot," 126.

³¹ For Vihiers, see *Cartulaire de Saint-Aubin*, no. 85; for Nouâtre, see note 18, above; and, for Loudun, see Louis Charbonneau-Lassay, "Les Châteaux de Loudun après les fouilles archéologiques de M. J. Moreau de la Ronde," *Mémoires de la Société des Antiquaires de l'Ouest*, 3d ser., 8 (1915): 76–93, 143–73; P. de Monsabert, ed., "Documents inédits pour servir à l'histoire de l'abbaye Sainte-Croix de Poitiers et de ses domaines jusqu'à la fin du XIII^e siècle," *Revue Mabillon* (1913), no. 1. Geoffrey recruited Alberic for Vihiers while returning home after

weakest part of Geoffrey's route was the fifty-kilometer stretch between Vihiers and Loudun; paradoxically, this was the area most threatened by the enemy in the Saumurois. When Geoffrey chose to establish Alberic at Vihiers, however, the Angevin count was a close ally of Viscount Aimery, whose stronghold at Thouars was twenty-two kilometers west of Loudun and twenty-eight kilometers southeast of Vihiers.³²

When Viscount Aimery of Thouars defected from the Angevin cause in 994,³³ Fulk found the protected southern route that Geoffrey had developed was seriously endangered. The distance between Vihiers and Loudun was too long to be negotiated safely in a day by a relief column that could also remain battle-ready. The region was easily penetrated by the newly hostile men of Thouars from the south and by the men of the Saumurois from the north. Under the pressure of this dual threat, Fulk was apparently willing to abandon his indirect strategy and seize the opportunity offered by Odo I's death to attack Tours in 996. If Fulk had been able to control the Loire from Montsoreau to Tours, he would not have had to bypass the Saumurois along the newly endangered southern route; instead, he could have traveled to Langeais and Tours by the northern route through Baugé (without having to go so far out of his way to the east as Amboise) and then turn south toward the valley of the Indre and Loches.

When Fulk lost Tours in 997, however, he needed to find another safe way to reach Loudun from Vihiers without having to establish camp for the night in hostile territory or push his horses to the point where they would not be useful in combat. Fulk probably compensated for the problem posed by the hostility of the viscount of Thouars by building the stronghold at Passavant to the east of Vihiers and by relying for protection on the old *confugium* and *fosse* at Montaglan only twenty-five kilometers west of Loudun and eighteen kilometers east of Passavant.³⁴ This route, while certainly not as direct as Fulk might have liked, provided a

supporting Hugh Capet at Montreuil-sur-Mer; for the date of that conflict, see Lot, *Les Derniers Carolingiens*, 116. On Loudun, also see Ademar de Chabannes, *Chronique*, ed. Jules Chavanon (Paris, 1897), Bk. III, chap. 36; Fulk Réchin, *Fragmentum historiae Andegavensis*, in *Chroniques des comtes d'Anjou*, 233; as well as Norgate, *England under the Angevin Kings*, 139–40; Halphen, *Le Comté d'Anjou*, 7, 55; Alfred Richard, *Histoire des comtes de Poitou, 778-1204*, 1 (Paris, 1903): 115–16; Marcel Garaud, *Les Châtelains de Poitou et l'avènement du régime féodal XI^e et XII^e siècles* (Poitiers, 1964), 3; Guillot, *Le Comte d'Anjou et son entourage*, 1: 5–8; and Bachrach, "A Study in Feudal Politics," 14.

³² Louis Halphen and Ferdinand Lot, eds., *Recueil des actes de Lothaire et de Louis V, rois de France* (Paris, 1908), no. 62. The editors considered this act inauthentic from a diplomatic point of view; Guillot agreed, but he made a compelling argument for accepting the accuracy of the material found in the document; *Le Comte d'Anjou et son entourage*, 1: 6 n. 28. For Angevin relations with the viscounts of Thouars, see George Beech, *A Rural Society in Medieval France: The Gâtine of Poitou in the Eleventh and Twelfth Centuries* (Baltimore, 1964), 129–30; and, for an important connection between Loudun and Thouars, see George Beech, "The Origins of the Family of the Viscounts of Thouars," in *Études de civilisation médiévale, IX^e–XII^e siècles, offertes à Edmond-René Labande* (Poitiers, 1974), 25–31.

³³ BN, MS Lat. 17,127, p. 156, and Coll. D. Houss. H.1, no. 352. For the date, see Guillot, *Le Comte d'Anjou et son entourage*, 2: no. 9.

³⁴ Fulk originally constructed a stronghold at Passavant in ca. 996–97, but it was not overly secure and was destroyed in 1010; Fulk Réchin, *Fragmentum historiae Andegavensis*, 234; and Hugh de Flavigny, *Chronicon*, in *Recueil des historiens des Gaules et de la France*, 207. Fulk could not have built Passavant earlier than 996 for the same reasons that neither Montbazou nor Mirebeau could have been constructed before that date; Bachrach, "Fortifications and Military Tactics," 531–49. Concerning the *confugium* and *fossa*, see *Cartulaire de Saint-Aubin*, no. 40.

modicum of protection in an otherwise untenable situation. Ideally, of course, Fulk might have preferred to establish not only a more direct route but also a route based on a major stronghold. Had Fulk ventured to build such an important connecting link further south, it would have been more vulnerable to attack from Thouars and more difficult to defend. Later in his reign, under the distinctly more favorable conditions that obtained following his capture of Saumur, Fulk strengthened his southern route by building a stronghold at Montreuil-Bellay—eighteen kilometers east of Passavant and only twenty kilometers northwest of Loudun. The garrison of *caballarii* at Montreuil-Bellay was specifically charged with the task of protecting the area from attacks by the men of Thouars.³⁵

Not all of Fulk's defensive needs were as extensive as those required for the protection of these northern and southern routes between Angers and Amboise. Following the loss of Tours in 997, for example, Fulk built a formidable stone *castrum* only eleven kilometers down the Indre river from—and on lands belonging to—the monastery of Cormery. Troops stationed at this stronghold at Montbazou were in a sound tactical position, because of excellent interior lines of communication, to block forces based at Tours from attacking the monastery and its holdings. Fulk could thus protect the considerable control he exercised over the fisc of Cormery.³⁶ In addition, Montbazou helped Fulk block the potential attack route to Loches from the northwest. The position of this stronghold at Montbazou—like the location of Morand north of Amboise—also had offensive strategic potential. Forces stationed at Montbazou were very well placed to interdict communications between Tours and Odo II's fortification at Île-Bouchard. A properly coordinated operation that included forces stationed at Langeais and at Montbazou could also cut off communications between Tours and Odo II's stronghold at Chinon.³⁷ Thus Montbazou had the potential to be used to hinder Odo's control of the lower valley of the Vienne. And troops from Montbazou, acting in concert with the garrison at Amboise, could easily cut communications between Tours and the lower valley of the Cher.

Given the diplomatic situation that obtained when Fulk built Montbazou, and given his adherence to what may be considered an indirect strategy following the loss of Tours in 997, the Angevin count probably did not intend to use this new stronghold as a base from which to sustain an immediate campaign against Tours. But Montbazou can also be seen as a link in a chain of strongholds encircling Tours: Amboise up river, Langeais and Maillé down river, and Semblançay to the north were all part of this circle. Fulk's primary reasons for building both Langeais and Semblançay were to insure communications between Angers and his holdings to

³⁵ An important act records the circumstances under which Fulk built Montreuil-Bellay and installed Berlais as castellan there ca. 1030; *Cartulaire de Saint-Aubin*, no. 220; and BN, N.A. Lat. 1930, ff. 107^v–108^r. Guillot failed to recognize that the "antique custom" referred to in the cartulary for the defense of this stronghold and the deployment of the garrison dates to the reign of Fulk Nerra; *Le Comte d'Anjou et son entourage*, 1: 390–91.

³⁶ For the date, see Bachrach, "Fortifications and Military Tactics," 540–41; and, for Cormery more generally, see note 21, above.

³⁷ For Odo's possession of Île-Bouchard, see Halphen, *Le Comté d'Anjou*, 42, 165; and, for Chinon, see *ibid.*, 19, 49.

the east; Fulk did not, of course, have a role in building Amboise, and the origins of Maillé are unknown.³⁸ But, despite what seem to have been his obvious defensive reasons for building Montbazou, Fulk quite possibly chose the site for its construction fully aware of the many tactical and strategic offensive options it provided.

Just as Montbazou was probably built primarily to protect Angevin control of the monastery of Cormery, Montrésor—which the Angevin count constructed at about the same time and placed under the command of Roger the Devil—seems to have been intended to protect the monastery of Villeloin five kilometers to the southeast on the Indrois.³⁹ Villeloin, whose fisc Fulk dominated, was located only eighteen kilometers southwest of Odo's stronghold at Saint-Aignan, a stronghold that helped give the count of Blois, in addition, control of the middle reaches of the Cher valley east to the monastery at Vierzon. The garrison at Saint-Aignan was clearly in a position to despoil Villeloin and its lands. In looking at a map, it appears that troops from the Angevin stronghold at Villentrois, ten kilometers southeast of Saint-Aignan and nineteen kilometers northeast of Villeloin, were in a position to interdict enemy raiders. But the route between Saint-Aignan and Villeloin passed through the great forest of Brouard. The terrain therefore favored the invaders, who could choose the time and place for their attacks; the would-be defenders stationed at Villentrois were severely hampered by the terrain in their efforts to hunt down the enemy.⁴⁰ By building a stronghold at Montrésor, Fulk provided direct protection for the monastery and lands of Villeloin.

The essentially defensive nature of Montrésor was given an added dimension a few years later when Fulk built a stronghold at Montrieux on the Cher, only twenty kilometers down river from Saint-Aignan. With Villentrois, Montrésor, and Montrieux all in place, Saint-Aignan was cut off on three sides; its only open line of communication was north to Blois, thirty-eight kilometers away. When construction at Montrieux was complete, Fulk placed it under the command of Roger the

³⁸ Halphen, *Le Comté d'Anjou*, 2, 15, 158. The earliest evidence for a stronghold at Maillé is found in an act dated between 1028 and 1037, when a castellan named Gausbertus can be identified as the commander. By 1037, moreover, when Odo II was killed and Langeais was once again safely in Fulk's hands, Maillé, we can assume, was also under Angevin control. See *ibid.*, 165; and Guillot, *Le Comte d'Anjou et son entourage*, 1: 464.

³⁹ Fulk Réchin, *Fragmentum historiae Andegavensis*, 234. Fulk built Montrésor before 1005, because in that year Roger, who held Montrésor, was established in the newly constructed stronghold of Montrieux; see note 41, below. Fulk could not have built Montrésor before 997; Bachrach, "Fortifications and Military Tactics," 547–49. Nor could he have done so during the period from September 1003 to December 1004, because during that time he made his first pilgrimage to the Holy Land. For the chronological limits of Fulk's pilgrimage, see *Cartulaire de Saint-Aubin*, no. 130; and the act published in Jean Besley, *Histoire des comtes de Poitou et ducs de Guyenne* (Paris, 1647), 357. For the dating of both of these, see Guillot, *Le Comte d'Anjou et son entourage*, 2: nos. 21, 22. Also see, for a discussion of the pilgrimage, Halphen, *Le Comté d'Anjou*, 213–18.

⁴⁰ For Villeloin, see note 21, above; for Odo I's possession of Saint-Aignan, see Halphen, *Le Comté d'Anjou*, 48; and, for Vierzon, see Devailly, *Le Berry*, 133–35. The extensive Forêt de Brouard, which today dominates the boundary between the departments of the Indre and the Indre-et-Loire for about twelve kilometers and at points between Saint-Aignan and Villeloin has a depth of six kilometers, is but a remnant of what it was during the Middle Ages. My travels in the area during the summer of 1981 left the impression that, despite much modern intervention, parts of the forest were still very thick and relatively untouched. For the present extent of the forest, see Michelin, *Carte au 2000.000ème*, no. 64, between 1.00 gr and 1.20 gr and north from 52.40 gr. The only work on the forests of this area for the medieval period is of no help: J. Martin-Déménil, "Les Forêts du comté de Blois jusqu'à la fin du XV^e siècle," *Mémoires de la Société des Sciences, Lettres, et Arts de Loir-et-Cher*, 34 (1963): 127–236. But Lynn Nelson has discussed a similar tactical problem for Fulk's near contemporaries across the Channel; see Nelson, *The Normans in South Wales, 1070–1171* (Austin, Texas, 1966), 116–17.

Devil, who had earlier been established as castellan at Montrésor.⁴¹ This posting suggests that Fulk may have been trying to coordinate Angevin strategy in the region, perhaps specifically against Saint-Aignan.

Such a dual-purpose—defensive and offensive—Angevin strategy is illustrated by Fulk's expansion east of the Morand-Amboise-Loches line. When the count placed Lisoius in overall command of the region southeast of Amboise in 1015, a second "frontier" of sorts had already been established to the east, from Montrichard on the Cher twenty-one kilometers south to Montrésor on the Indrois and twenty kilometers further south to Châtillon-sur-Indre. In the southernmost part of this region the Angevin push east was furthered by their longstanding control of Villentroy and the recruitment of Renaud of Graçay as a *fidelis*, whose stronghold was only thirty kilometers east of Villentroy along the route to Bourges.⁴² Northeast of Montrésor the Angevins captured the castellan of Saint-Aignan, and Fulk gave Léon of Meung-sur-Loire, whose fortification lay ten kilometers down the Loire from Orléans, responsibilities in Amboise, charging Léon with pressing Angevin expansion north of the Cher.⁴³

As a local chronicler observed, Fulk found the strongholds of Beaugency and Meung-sur-Loire valuable links for his communications in the Orléanais.⁴⁴ This "defensive" view of Fulk's recruitment of *fideles* in this region is probably accurate in the short term. But Fulk was undoubtedly aware that these two strongholds controlled the Loire valley east of Blois. These fortifications, along with the Angevin stronghold at Château-Landon, which Fulk brought back under his control ca. 1029,⁴⁵ seriously hampered Odo II's communications with his eastern lands and lordships. Meung-sur-Loire and Beaugency up the Loire from Blois and Amboise down river from Blois effectively bracketed Odo's river access east and west, including Blésois access to Tours. In connection with Angevin domination of Vendôme, which stood between the count of Blois and his northern lordships, and with Angevin control of the stronghold at Montrichard (Odo's projected campaign against Montrichard in 1016 had brought about the battle of Pontlevoy), which cut Odo off from access to the southwestern Touraine, Fulk's fortifications point to a

⁴¹ For the construction of Montrichard, see *Chronicon Turonense Magnum*, in A. Salmon, ed., *Recueil des chroniques de Touraine* (Tours, 1854), 117; and *Chronicon Turonense Abbreuiatum*, *ibid.*, 187. For a good discussion of the date, see Halphen, *Le Comté d'Anjou*, 31–32 n. 3. Also see note 39, above.

⁴² On Graçay, see L. Raynal, *Histoire du Berry*, 1 (Bourges, 1844): 350 n. 2, 424 n. 1. Raynal published fragments of an act that Guillot dated; *Le Comte d'Anjou et son entourage*, 2: no. 15. For Lisoius's command, see *Chronica de gestis consulum Andegavorum*, 48, 54; and *Gesta Ambaziensium Dominorum*, 79, 84.

⁴³ For the installation of Léon, which occurred after the death of Sulpicius in 1027, see *Gesta Ambaziensium Dominorum*, 89–90; and Jacques Boussard, "Le Trésorier de Saint-Martin de Tours," *Revue d'histoire de l'Église de France*, 47 (1961): 78. For the castellan of Saint-Aignan, see *Gesta Ambaziensium Dominorum*, 80. The stronghold itself appears to have held out until 1044; Halphen, *Le Comté d'Anjou*, 49 n.; and Guillot, *Le Comte d'Anjou et son entourage*, 1: 63–65 n. 281.

⁴⁴ *Gesta Ambaziensium Dominorum*, 90.

⁴⁵ Fulk regained a strong position at Château-Landon through the marriage of his daughter Blanche-Hermengarde to Count Geoffrey; Halphen, *Le Comté d'Anjou*, 11 n. 1, 12 n. Yet scholars have failed to recognize that Fulk gained the vicecomital office at Château-Landon as a result of this union; *Gesta Ambaziensium Dominorum*, 90. Halphen and Pourpardin, the editors of the chronicles of the counts of Anjou, mistakenly identified the Count Fulk who held the *viceconsulatum* as Fulk Réchin rather than Fulk Nerra; *Chroniques des comtes d'Anjou*, 269. Fulk Réchin did, of course, hold Château-Landon, but at a later time. Angevin influence at Château-Landon postdated the marriage, which took place sometime after 1029; P. Marchegay, ed., *Cartulaire du Ronceray* (Angers, 1856), nos. 125, 126.

strategy of encirclement that was worked out over a period of twenty-five years. That strategy only became operative during the two or three years following Odo's unsuccessful attack on Amboise in 1026, when Fulk's careful acquisition and construction of strongholds since the late 990s paid off.⁴⁶

On the western frontier, the combined defensive-offensive strategy of the Angevin count is no less apparent. Control of Nantes and defense of the Loire valley east to Angers was a policy that Fulk inherited from his father, a policy Geoffrey Greymantle had pursued vigorously.⁴⁷ To sustain this strategy, Fulk used his influence to acquire the stronghold of Champtoceaux for his *fidelis* Renaud of Thorigné ca. 987–88, installed his own *vassalus* Drogo at Châteaupanne ca. 1006, and shortly thereafter arranged for the construction of a stronghold at Montjean.⁴⁸ This effort was followed up with yet another complex of fortifications at Saint-Florent-le-Vieil.⁴⁹ The defensive significance of this series of strongholds along the Loire between Angers and Nantes is obvious, but in an offensive sense the Angevins could be seen to strengthen their claims of overlordship in the Nantais. Indeed, from time to time the counts of Nantes had proved to be reluctant *fideles* of the Angevins by revolting against their overlords.⁵⁰ These strongholds provided Fulk with a secure attack route to “reconquer” the region and reimpose his overlordship.

In addition to providing for Angevin control of the Loire valley to the west of Angers, the strongholds at Châteaupanne, Montjean, and Saint-Florent-le-Vieil firmly established Angevin control of the northern part of the Mauges region. Fulk the Good, Fulk Nerra's grandfather, had laid a basis of sorts for Angevin claims in the Mauges, and Geoffrey Greymantle's *conventiae* with Renaud of Thorigné had established Angevin comital rights to substantial alodial lands in the region; Fulk Nerra executed the stipulations of the *conventiae* in 1005 and built the stronghold of Montrevault to defend his occupation of the land south of the Loire.⁵¹ He followed up Montrevault with the construction of fortifications at Chemillé prior to 1016 and Beaupréau prior to 1029, integrating more fully the Mauges into his dominion and giving him six strongholds in the region. Then, to the south, he built Montfaucon ca. 1026 and perhaps la-Tour-Landry, which established a defended frontier

⁴⁶ For Pontlevoy, see Halphen, *Le Comté d'Anjou*, 34; and, for Amboise and its aftermath, see *ibid.*, 39; *Annales Vindocinenses*, 60; and *Annales qui dicuntur Rainaldi archidiaconi Sancti Mauricii Andegavensis*, 86, in L. Halphen, ed., *Recueil d'Annales Angevines et Vendômois* (Paris, 1903), 86.

⁴⁷ Bachrach, “Robert of Blois, Abbot,” 127–28, and “The Idea of the Angevin Empire,” 295.

⁴⁸ For Renaud, see Hogan, “The Rainaldi of Angers,” 35–62; and Bachrach, “Robert of Blois, Abbot,” 127–28; for Drogo, see *ibid.*, 139–40; and BN, N.A. Lat. 1930, ff. 26v–27v; and, for Montjean, see Halphen, *Le Comté d'Anjou*, 157 n. 4.

⁴⁹ Halphen dated the construction to ca. 1030, but Guillot placed the date at 1036 or 1037; *Le Comté d'Anjou*, 52, 155, 291; and *Le Comte d'Anjou et son entourage*, 1: 230 n. 133. A date close to 1033 is in order not only because of the role played by Countess Agnes but also because of manuscript evidence from the Bibliothèque Nationale. That manuscript makes it clear that, by 1061, at least part of the fortifications at Florent-le-Vieil had been in existence for more than twenty-five years; BN, N.A. Lat. 1930, ff. 57r–58r. Chronicle evidence illuminates the complexity of these fortifications, which were constructed partially of wood and partially of stone; *Historia Sancti Florentii*, 282.

⁵⁰ Bachrach, “Robert of Blois, Abbot,” 126–27; and Guillot, *Le Comte d'Anjou et son entourage*, 1: 8–12, 42, 209. Concerning the revolts, see Halphen, *Le Comté d'Anjou*, 51–52; and Bachrach, “Fulk Nerra and His Accession,” 332.

⁵¹ For Fulk the Good, see R. Merlet, ed., *Chronique de Nantes* (Paris, 1896), chap. 37; Lot, *Les Derniers Carolingiens*, 347; Halphen, *Le Comté d'Anjou*, 5; and Guillot, *Le Comte d'Anjou et son entourage*, 1: 10 n. 5; for Geoffrey's *conventiae*, see Hogan, “The Rainaldi of Angers,” 36–38 (which supplants Guillot, *Le Comte d'Anjou et son entourage*, 1: 215–17); and, for Montrevault, see Guillot, *Le Comte d'Anjou et son entourage*, 1: 227–29.

against the viscounts of Thouars.⁵² These two strongholds also provided the western links to the “limes” that ran east to Vihiers, Montreuil-Bellay, and Loudun into the Touraine.⁵³

The “limes” on Fulk’s southern frontier demonstrate additional complexities inherent in his defensive-offensive strategy. During the first decade and a half of Fulk’s reign, for example, the stronghold at Loudun was of great importance not only because it provided a vital link in the route between Vihiers and Passavant to the west and Nouâtre to the east but also because it dominated the direct route from Poitiers to Saumur. But, with the defection of the viscount of Thouars in 994, the accession of William the Great as count of Poitou in 995, and the loss of Tours to King Robert in 997, Loudun was seriously threatened by encirclement. Thus, when Fulk constructed a stronghold at Mirebeau, twenty-six kilometers to the south, sometime between 997 and the turn of the century, the Angevin count substantially reduced the threat from Poitiers.⁵⁴

From two perspectives Mirebeau was primarily defensive. Forces stationed at Mirebeau were in a position to harass William’s lines of communication and supply if the count launched an offensive against Loudun further to the north. Indeed, William would have had to reduce Mirebeau before he could make a serious effort against Loudun. Since Mirebeau was less than a day’s travel from Loudun, forces stationed at Loudun could operate in the field to weaken the effort of those conducting a siege at Mirebeau. The position of Mirebeau, then, clearly demonstrates the reciprocal defensive strength of two or a cluster of strongholds within less than a day’s travel of one another. Mirebeau’s second important defensive role lay in its position adjacent to the lands and estates belonging to the monastery of Cormery. Thus, by building this stronghold, Fulk strengthened his control over

⁵²Sigebrannus, Fulk’s standard bearer at the battle of Pontevoy, was already in possession of Chemillé by 1016; *Historia Sancti Florentii*, 274; and Halphen, *Le Comté d’Anjou*, 157–58. For the date of Beaupréau, see *ibid.*, 156, 157, 163. Guillot took issue with Halphen on this point, but Guillot’s argument is not convincing; *Le Comté d’Anjou et son entourage*, 1: 299–300. For Montfaucon, see Halphen, *Le Comté d’Anjou*, 155. La-Tour-Landry, located south of Chemillé, was an important fortification of the later Roman empire; R. Favreau *et al.*, eds., *Atlas historique français: Anjou* (Paris, 1970), map. 111.3. Geoffrey of la-Tour-Landry, who was the son of Landry and thus perhaps the son of the first castellan, flourished during the second half of the eleventh century; chronologically, la-Tour-Landry could well have been one of the many strongholds that Fulk Nerra built but that are only alluded to by Fulk Réchin; *Fragmentum historiae Andegavensis*, 243; and Guillot, *Le Comté d’Anjou et son entourage*, 1: 462.

⁵³The use of such terms as “limes” and “frontier” may appear controversial and perhaps even anachronistic to some medievalists. Both of these terms, however, have wide application. See, for example, the many instances for the use of “limes” cited in Edward Luttwak, *The Grand Strategy of the Roman Empire* (Baltimore, 1976), 66–67, 78–79, 96–97, 176–77, 208–09, 211–12, 214, 228. Of particular interest in the present context is the notion of “open limes,” in which the road connections between strongholds are singled out for attention; *ibid.*, 61. For the use of the term “limes” and a concentration on frontier defense, see Philippe Contamine, *La Guerre au moyen âge* (Paris, 1980), 366–69. Of substantial importance are the studies of Jean Hubert, who has shown how the old Roman road was used by the counts of Blois-Champagne to create a frontier and has discussed “routes de château à château”; Hubert, “La Frontière occidentale du comté de Champagne du X^e au XIII^e siècle,” in *Recueil de travaux offerts à M. Clovis Brunel* (Paris, 1955), 3–19, and “Les Routes du moyen âge,” in his *Les Routes de France* (Paris, 1959), 35–36. A strategy based on routes from one château to another was designed to ensure communications between strongholds on the frontier and the central fortification of the seigneurie, held by the magnate who created the system. Hubert found one such system of “routes de château à château” in Berry, contemporary with the reign of Fulk Nerra. And documents from Fulk’s reign present, for example, stipulations concerning “castello faciendis in marchia”; C. Métais, ed., *Cartulaire de la Trinité-de-Vendôme* (Paris, 1893), no. 92. (Guillot failed to note Fulk’s role here; *Le Comté d’Anjou et son entourage*, 2: no. 119.) For “castrum faciendi,” see Mailfert, “Saint-Nicholas d’Angers,” p.j. no. 4.

⁵⁴Bachrach, “A Study in Feudal Politics,” 115, and “Fortifications and Military Tactics,” 147–49.

Cormery's fisc and also provided the means to obtain the labor to construct and maintain these fortifications.⁵⁵ Quite obviously, he thereby solved his problem of supply as well.

The Angevins did not originally establish Loudun for defensive purposes. Rather, Geoffrey Greymantle had built a formidable stone tower in the old Roman *castrum* at Loudun to extend Angevin power south of the Loire at the expense of the count of Poitou. Following his acquisition of Loudun, Geoffrey obtained extensive rights in the region over the lands and dependents of the convent of Sainte-Croix. The abess of Sainte-Croix granted Geoffrey rights (*bidannum*) that enabled him to maintain the stronghold and supply his garrison in return for "protection" for the convent and its lands.⁵⁶

This policy of usurpation and extortion masked by an oath of faithfulness and buttressed by building strongholds and providing protection was continued by Fulk Nerra, who constructed fortifications not only at Mirebeau but also at Faye-le-Vineuse and Moncontour, which dominated the monastery of Saint-Jouin-de-Marnes. The cluster—Loudun, Moncontour, Mirebeau, and Faye-le-Vineuse—provided a very slightly biased rectangular deployment of strongholds that established Angevin domination of the region between the Dive and the Veude and gave the Angevins a spearhead for penetrating Poitou; the southernmost point of this rectangle, Mirebeau, reached within twenty-six kilometers of Poitiers itself.⁵⁷ Although in 999 Fulk did recognize that he held Loudun and several other strongholds as *beneficia* from William of Poitou, this arrangement had no practical significance in limiting the activities of the Angevin count. The canons of Saint-Hilaire recognized the need for Angevin protection in this region, much as had the abess of Sainte-Croix, and thus provided additional resources for Fulk's exploitation. And, by 1033, those concerned recognized that much of the region between the Dive and the Veude formed part of the Angevin *comitatus*.⁵⁸

As Fulk's efforts to extend his domination moved further from his central

⁵⁵ Bachrach, "Fortifications and Military Tactics," 542–43.

⁵⁶ Bachrach, "A Study in Feudal Politics," 114 n. 11.

⁵⁷ For Faye-la-Vineuse and Moncontour, see Halphen, *Le Comté d'Anjou*, 154, 161–62. The monastery of Saint-Jouin-de-Marnes is less than a bowshot from the tower at Moncontour and could have been dominated completely by the garrison at the stronghold. As long as Fulk's garrison was on good terms with the monks, any attack on the monastery would have been less than prudent. For Fulk's good working relations with the abbots of Saint-Jouin, see L. de Grandmaison, ed., *Cartulaire de Saint-Jouin-de-Marnes* (Niort, 1854), 19. Since Fulk's brother-in-law controlled the fortifications at Chéneché to the south of Mirebeau, Fulk's effective control extended much closer to Poitiers than the location of the Mirebeau rectangle in itself indicates. Carl von Clausewitz in the nineteenth century pointed to the "extent to which geometry, or form and pattern in the deployment of forces in war, can become a dominant principle." Geometry, he said, "applies to almost everything, large or small." Fortification in the geometric sense, however, does not necessarily mean a single fortification but interrelated structures, fortifications that can be used to hold threatened territory. In this context, the use of groups of strongholds is superior to the use of solid lines, like Hadrian's Wall. Clausewitz, *On War*, ed. M. Howard and P. Paret (Princeton, 1976), 214–15, 409–14, 484–87.

⁵⁸ The cartulary of the monastery provides evidence for an act ("ex antiquis . . . scriptis") by which the monks of Saint-Hilaire granted various lands and rights to Fulk Nerra in the region of Loudun; the region is described as part of the *comitatus* of Count Fulk. The act refers to great violence in the region and the need to have the count's protection. Since Fulk was at peace with William the Great until his death in 1030 and the only major conflict in the region took place during the reign of William the Fat, it is to 1033 that this act should be assigned. L. Redet, ed., *Cartulaire de Saint-Hilaire de Poitiers* (Poitiers, 1847), no. 133; and Bachrach, "A Study in Feudal Politics," 120–21. On the *beneficia*, see Bachrach, "William the Great," 15, and "A Study in Feudal Politics," 116–21.

frontiers (the northern and southern routes between Amboise and Angers and their natural extensions), the proportion of strongholds that he built decreased and of those over which he gained control through various other means increased. For example, Fulk obtained Saintes and various strongholds in the Saintonge as *beneficia* from William the Great in the settlement of 999. Subsequently, the Angevin count began to develop a route of communication to the south. First, in the Gâtine of Poitou he gained control of Parthenay, which he fortified prior to 1012.⁵⁹ During the remainder of his reign, Fulk came to dominate a line of strongholds that extended from two *foci* of Angevin power in the north—Loudun and Angers—southward to Saintes. One line, from Loudun, reached south to Moncontour, Parthenay, Germond, Mauzé, Tonnay-Boutonne, and Taillebourg, and on to Saintes. The second, from Angers, ran to Rochefort-sur-Loire, Chemillé, Maulévrier, perhaps Bressuire, and Parthenay, coinciding at that point with the Loudun route to Saintes. The greatest distance between any of these fortified links on the two routes was the thirty-five kilometers separating Maulévrier and Parthenay, and the average distance was less than twenty-three kilometers.

South of his frontier, Fulk may have participated in the building of Parthenay, the Angevins clearly joined William of Parthenay in constructing Germond, and Mauzé's lord, also named William, was an Angevin ally.⁶⁰ Mascelinus of Tonnay-Boutonne is known to have held a prestigious *beneficium* at Saintes from the Angevins, and Taillebourg, just to the north, was initially constructed by Aimery II of Rancon, one of Fulk's supporters. Another of Fulk's men, the count of Angoulême, destroyed Taillebourg, but then rebuilt it.⁶¹ On the route south from Angers, Fulk built Maulévrier. The origins of Bressuire are unknown, but it, like the fortification at Tonnay-Boutonne, shared several key stylistic similarities with Fulk's strongholds at Loudun and Langeais.⁶²

⁵⁹ For the Saintonge, see Bachrach, "A Study in Feudal Politics," 119–20; and, for Parthenay in particular, see Beech, *A Rural Society in Medieval France*, 129–31. Martindale's objection to the existence of very close relations between Joscelin of Parthenay and the Angevins seems wide of the mark; for her argument, see "Conventum," 533.

⁶⁰ Martindale has provided conclusive evidence that Fulk played a crucial role in determining who held Parthenay; "Conventum," 542. For Mauzé, see J. Verdon, ed., *La Chronique de Saint-Maixent, 751-1140* (Paris, 1979), 118. The Saint-Maixent chronicle also places the building of Germond by the men of the Poitevin Gâtine, aided by the Angevins, after the death of William the Fat but in the same time frame, "Eodem tempore." Since William the Fat died on December 15, 1038, and his brother died on March 10, 1039, after first laying siege to Germond and then to Mauzé without taking either one, it seems likely that the chronicle has telescoped the sequence of events and placed the construction after the death of William when it should be placed before his death; for the chronology, see Richard, *Histoire des comtes de Poitou*, 233, 236–37. In other words, it is unlikely that the Angevins and the men of the Gâtine learned of William the Fat's death in the days following December 15, developed a plan to build a stronghold at Germond, gathered the necessary equipment and workers in mid-winter, rushed south to raise a *motte* and dig a moat, organized a garrison, and withstood a lengthy siege by Odo, William the Fat's brother, that lasted until late February or early March of 1039; for the topography, see *ibid.*, 236 n. 1. Germond must, therefore, have been planned and begun before mid-December 1038, although it may not have been fully completed until after that date.

⁶¹ T. Grasilier, ed., *Cartulaire de Notre-Dame de Saintes* (Noirt, 1871), nos. 1, 17; and Ademar, *Chronique*, Bk. IV, chap. 60. For the identification of "Fractum Botum," which the count of Angoulême destroyed, as Taillebourg, see Jacques Boussard, ed., *Historia Pontificum et Comitum Engolismensium* (Paris, 1957), chap. 25 n. 1.

⁶² Halphen, *Le Comté d'Anjou*, 154, 161; and Garaud, *Les Châtelains de Poitou*, 19. Garaud speculated that the viscounts of Thouars were responsible for the building of the stronghold of Bressuire. Since Parthenay was only twenty-eight kilometers and Faye-l'Abbesse, another Angevin possession, was only eleven kilometers from Bressuire (Thouars was twenty-six kilometers away), Bressuire's origins are more likely Angevin, especially

The route to Saintes south of Parthenay, like Fulk's other routes, probably had both defensive and offensive purposes. Clearly, if Fulk wanted to maintain an effective Angevin presence at Saintes, he had to be in a position to support his garrison there in the event of an enemy siege. The stronghold at Tonnay-Boutonne was only fifteen kilometers west of the very important and wealthy monastery of Saint-Jean-d'Angély, where William of Poitou experienced serious difficulty in maintaining his authority. Fulk did not hesitate to advise that the monastery be radically reorganized.⁶³ In addition, the monasteries at Maillezais and at Saint-Maixent were both potential victims of troops stationed at the Angevin strongholds on the route to Saintes. It seems reasonable to suggest that Fulk would not have been loath to create the same kinds of conditions for these houses that had shown the nuns of Sainte-Croix and the canons of Sainte-Hilaire the wisdom of obtaining "protection" from the Angevins.

Also from the offensive point of view, the north-south line of strongholds from Parthenay to Saintes linked up with Fulk's east-west "limes" and the Loudun cluster (including Moncontour, Faye-le-Vineuse, and Mirebeau), which partially encircled William of Poitou's heartland. This strategy of encirclement was greatly strengthened by Fulk's alliance with Count William of Angoulême, who married his sister, Gerberga. Initially Fulk's brother-in-law and later his nephews controlled Chéneché to the north of Poitiers and the strongholds at Melle, Aulnay, Ruffec, Chabanais, Rochechouart, and Confolens to the south.⁶⁴ In addition, Fulk acquired as *fideles* Aimery I and his son Aimery II of Rancon and Hugh of Lusignan. Between them these magnates controlled not only Rancon and Lusignan but also the fortifications at Chivray, Chizé, *Malavallis*, and Gençay. As Fulk's *fideles*, they were obligated to place the fortifications they held in the Angevin count's hands whenever he required.⁶⁵ Through this strategy of building and acquiring strongholds Fulk Nerra thoroughly undermined the power of William the Great in Poitou and its environs. This strategy was largely indirect; after 999 Fulk took no overt hostile action against William. The Poitevin, who was apparently more interested in legal theory than in military strategy, seems to have been satisfied that Fulk recognized him as his *senior* for various *beneficia* and either did not recognize the significance of Angevin encroachment or was powerless to thwart it.⁶⁶

When William died, the Angevins shifted from an indirect to a direct strategy. Geoffrey Martel married Agnes, the widow of William the Great. Soon thereafter, Geoffrey defeated the new count of Poitou, William the Fat, Agnes's stepson, at the

given the archaeological evidence. The only history of Bressuire provides no sound data on its foundation; B. Ledain, *Histoire de Bressuire* (Bressuire, 1880). For Faye-l'Abbesse, see Beech, *A Rural Society in Medieval France*, 130. And, for the structural similarities of these fortifications, see André Châtelain, *Donjons romans des pays d'Ouest* (Paris, 1973), 31.

⁶³ Ademar, *Chronique*, Bk. III, chap. 56. It is clear that Ademar is not telling the entire story, and his account is consistent with his efforts to cast William in the best possible light.

⁶⁴ Bachrach, "A Study in Feudal Politics," 119.

⁶⁵ Martindale, "Conventum," 543, 544, 546, 547; and Bachrach, "William the Great," 15–19.

⁶⁶ Concerning William's interest in the contractual obligations of the *fidelis* to his *senior*, see Fulbert, *The Letters and Poems of Fulbert of Chartres*, ed. F. Behrends (Oxford, 1976), no. 51; and, for William's noteworthy lack of military ability, see Bachrach, "William the Great," 12–15.

battle of Montcouëur. The younger William was imprisoned and, though later freed, died within a few years without ever becoming important on the political scene; Guy, William's brother, was killed the following year while opposing the Angevins. Thus, by the end of Fulk's reign when Geoffrey Martel held a great court at Vendôme, scores of Angevin supporters attended. Among the greater magnates from Poitou and its environs were the bishops of Poitiers, Angoulême, and Saintes, the abbots of Charroux, Bonneville, Saint Martial, and Saint-Jean-d'Angély, the count of Angoulême, the viscounts of Thouars and Châtellerault, and the lords of Rancon, Dols, Vouvant, la Roche, Verruy, Melle, Rochemeaux, and, of course, Parthenay.⁶⁷

The pursuit of an indirect strategy, intended to build up a secure position, followed by a more overt grab for power such as the Angevins employed in Poitou, had an analogue in the north. Fulk built and acquired strongholds for the purpose of extending his frontiers to the Loir and dominating Maine. Just as Geoffrey Grey mantle had laid the basis for Fulk's policy in Poitou, so too had he prepared the way in Maine. The viscounts of Le Mans and the house of Bellême were in Geoffrey's *mouvance*.⁶⁸ It seems that the Angevin count also had a claim of sorts to be overlord of the count of Maine and had some word as well as to who became bishop of Le Mans.⁶⁹ The alliance that Geoffrey arranged with Count Bouchard of Vendôme in 985 had been prepared by the recruitment of the viscount of Vendôme as a *fidelis*.⁷⁰ In the west, the Angevins controlled various estates in the neighborhood of the stronghold of Sablé, which was held by the brother of the

⁶⁷ For William the Fat and Guy, see Richard, *Histoire des comtes de Poitou*, 231–37; and Halphen, *Le Comté d'Anjou*, 57–60; and, for Geoffrey's great court, see *Cartulaire de la Trinité-de-Vendôme*, nos. 35–40. Several of these acts are not authentic in their present form and have been interpolated. It is likely, however, that the original witness lists have been preserved; see Hermann Meinert, "Die Fälschungen Gottfrieds von Vendôme," *Archiv für Urkundenforschung*, 10 (1928): 251–57.

⁶⁸ G. Busson and A. Ledru, eds., *Actus Pontificum Cenomannis in urbe degentium* (Le Mans, 1902), 352. Here the Angevin count is called "Fulco," which must be an error because Fulk the Good died in 960 and his son Geoffrey Grey mantle was count from 960 to 987. Seginfredus became bishop of Le Mans no later than February 971 but not before November 13, 968; R. Latouche, *Histoire du comté du Maine pendant le X^e et le XI^e siècle* (Paris, 1910), 134. For Geoffrey's relations with Viscount Radulf of Le Mans, see C. de Grandmaison, ed., "Fragments de chartes du X^e siècle provenant de Saint-Julien de Tours," *Bibliothèque d'École des Chartres*, 46 (1895): no. 21. The witness list was structured by the scribe in groups of names suggesting close relations among the members of each group, and Radulf appears in Geoffrey's group. For Geoffrey Grey mantle's relations with Geoffrey of Sablé, the son of Viscount Radulf of Le Mans, see *Cartulaire de Saint-Aubin*, no. 85; for Mainard, the brother of the viscount, see *Actus Pontificum Cenomannis*, 350; and, for the close relations of the two houses, see Jacques Boussard, "La Seigneurie de Bellême aux X^e et XI^e siècles," *Mélanges d'histoire du moyen âge dédiés à la mémoire de Louis Halphen* (Paris, 1951), 43–54. Also see Latouche, *Histoire du comté du Maine*, 127–31.

⁶⁹ By the twelfth century it was widely circulated in Anjou that Geoffrey Grey mantle had been granted the county of Maine by a grateful king of France in return for important military services. Achille Luchaire and Ferdinand Lot both suggested that the medieval evidence of the Angevin claim, *De Senescalcia Franciae*, was developed, at least in its present form, to justify Angevin domination of the region; Luchaire, "Hugh de Clers et le 'de senescalcia Franciae,'" *Bibliothèque de la Faculté des Lettres de Paris*, 3 (1897): 1–38; and Lot, *Études sur la règne de Hugues Capet et la fin du X^e siècle* (Paris, 1903), 197–98. Although the formal aspects of this account are undoubtedly fictitious, it is not impossible that King Lothair or perhaps Hugh Capet, who claimed overlordship of Maine, either individually or in concert conceded a "blank check" to Geoffrey so that he could aggrandize Angevin interests in the region; for this argument, see Norgate, *England under the Angevin Kings*, 140–42. Indeed, Geoffrey did support Lothair vigorously in the Norman war that began in 960 and again heroically against Otto II in 978, and he served with Hugh Capet at Montreuil-sur-Mer in 980 and at Marçon in 987; Fulk Réchin, *Fragmentum historiae Andegavensis*, 233; Guillot, *Le Comte d'Anjou et son entourage*, 1: 21; Lot, *Les Derniers Carolingiens*, 116; and *Cartulaire de Saint-Aubin*, no. 85. And Geoffrey's father had already encroached on Maine; see BN, Coll. Balluze 76, f. 256, and Coll. D. Houss. II.1, no. 358; and note 68, above.

⁷⁰ Fanning, "Les Origines familiales de Vulgrin," 253–54.

viscount of Le Mans, and fifty kilometers west of Sablé the Suhard-Warinus clan, long-time Angevin *fideles*, held the stronghold of Craon.⁷¹

Fulk worked on all fronts with the resources inherited from Geoffrey to further Angevin penetration of Maine. One of Fulk's priorities apparently was to secure a northwestern frontier that ran east from Craon to Sablé. Since the fifty-kilometer distance between these two fortifications created a situation that was logistically unacceptable (clearly it required an overlong day's travel for a force to move from one to the other), in 1007 Fulk saw to the construction of a *castellum* at Château-Gontier, which was twenty kilometers east of Craon and thirty kilometers west of Sablé.⁷² This formation of strategically interdependent strongholds constituted the Angevin "limes" in the northwest when viewed from a defensive perspective, but this line of fortified positions, when viewed from an offensive perspective, could serve as a base for further penetration of Maine. When Fulk recruited Lisoius, a military figure of substantial talent whose estates at Bazougers and Sainte-Christine were strategically located twenty-five kilometers north-northeast of Château-Gontier,⁷³ the Angevin count seems to have developed a spearhead—not unlike the rectangle of fortifications between the Dive and the Veude—pointing toward the important stronghold of Mayenne, only thirty-five kilometers further north.

The formation of this defensive-offensive "limes" appears to have been indirect. Craon was an old stronghold about ninety kilometers west of Le Mans. Château-Gontier was built on land belonging to the monastery of Saint-Aubin. Sablé had been built by the count of Maine, or perhaps by the viscount, and then was given to the brother of the viscount of Le Mans. And Fulk's recruitment of Lisoius ostensibly for service on the eastern frontier could hardly have been considered an overt act of hostility, despite the excellent location of his estates for providing logistical support for any Angevin strike further to the north. But shortly after Fulk had established contact with Lisoius, Angevin forces attacked and captured the stronghold at Mayenne.⁷⁴ This important stronghold could also be used as a base from which to threaten Norman interests to the north.⁷⁵

⁷¹ On Geoffrey of Sablé, see *Cartulaire de Saint-Aubin*, no. 85; and Latouche, *Histoire du comté du Maine*, 62. The stronghold at Craon was built during the ninth century; Guillot, *Le Comte d'Anjou et son entourage*, 1: 310. But it is difficult to ascertain just when it came under Angevin control. Suhardus the Old, who held Craon, first appears in the sources as part of Geoffrey's entourage in 965, at the latest, and Warinus is known to have been in the Angevin entourage at the same time; *Cartulaire de Saint-Aubin*, nos. 20, 34, 38, 48, 131, 281, 821; and *Cartulaire noir de Cathédrale d'Angers*, no. 18. I surmise that this Warinus and Suhardus were brothers-in-law. For the subsequent history of the Suhard-Warinus clan, see, for example, *Cartulaire de la Trinité-de-Vendôme*, no. 98, a text that shows the alternation of the *Leitnamen* Suhard and Warinus.

⁷² *Cartulaire de Saint-Aubin*, nos. 1, 677. Guillot declared that number 1 is a forgery, in contradistinction to Halphen, who merely saw it as suspect; *Le Comte d'Anjou et son entourage*, 2: no. 6; and *Le Comté d'Anjou*, no. 53. The interrelationship of numbers 1 and 677, however, makes clear that at least the military information found in the former can be trusted.

⁷³ *Gesta Ambaziensium Dominorum*, 76–77, 86.

⁷⁴ For Craon, see Guillot, *Le Comte d'Anjou et son entourage*, 1: 310; for Château-Gontier, see *Cartulaire de Saint-Aubin*, no. 677; for Sablé, see Latouche, *Histoire du comté du Maine*, 59; and, for the capture of Mayenne, see R. Charles and S. Menjot d'Albenne, eds., *Cartulaire de Saint-Vincent-du-Mans* (Mamers, 1886), no. 245; and Latouche, *Histoire du comté du Maine*, 54 n. 1. In discussing Mayenne, Guillot did not take into account the fact that Fulk imposed his lordship on Count Herbert of Maine at about this same time; *Le Comte d'Anjou et son entourage*, 1: 21, 457–58.

⁷⁵ Le Patourel, *The Norman Empire*, 304. The Normans were traditional enemies of the Angevins. One of Fulk's great uncles had been killed fighting them, Geoffrey Greymantle had fought them in 960, and Fulk

By 1014 Fulk controlled western Maine as far north as Mayenne. He also had very good relations with the lords of Bellême to the northeast, the family of the viscounts of Le Mans, and the bishop of Le Mans. Vendôme was in the *mouvance* of the Angevins, and Fulk enjoyed a very strong position on the Loir. Under these circumstances Fulk repeatedly required the counts of Maine to recognize his overlordship.⁷⁶ In 1025 Fulk sought to take direct control of the *comté*; although this initiative failed, the Angevins continued to press their efforts in Maine, especially at Le Mans, where *fideles* were recruited, the vicecomital family was favored, and direct control over the bishopric was pursued with vigor.⁷⁷ In addition, the Angevins maintained their policy of securing the valley of the Loir as the northern “limes” of the Angevin heartland. To this end they either built or gained control over already existing strongholds at Briollay, Matheflon, Durtal, La Flèche, Le Lude, Château-du-Loir, la Chartre-sur-le-Loir, and Lavardin.⁷⁸ Hardly surprising are the distances between the strongholds along this new frontier: the average

himself opposed them in 992, when they helped Conan of Rennes besiege Nantes. In addition, the Angevin alliance with the count of Vexin established by Fulk had strategic importance with regard to the Normans. The same may be said for Angevin efforts to maintain close ties with the lords of Bellême. See Lot, *Les Derniers Carolingiens*, 34, 166 n. 4, 346–57; Halphen, *Le Comté d'Anjou*, 21 n. 3; *Chronique de Nantes*, chaps. 37–39; Grierson, “L’Origine des comtes d’Amiens, Valois, et Vexin,” 96–97; Werner, “Untersuchungen zur Frühzeit des französischen Fürstentums,” 271; and note 65, above.

⁷⁶ See Orderic Vitalis, *Historia ecclesiastica*, Bk. IV, ed. Marjorie Chibnall, *The Ecclesiastical History of Orderic Vitalis*, 2 (Oxford, 1969): 304; and, more obliquely, Fulk Réchin, *Fragmentum historiae Andegavensis*, 233; and *Gesta Ambaziensium Dominorum*, 77. For discussion, see Norgate, *England under the Angevins*, 159–60; Halphen, *Le Comté d'Anjou*, 66–67; Latouche, *Histoire du comté du Maine*, 18, 22; and Guillot, *Le Comte d'Anjou et son entourage*, 1: 21.

⁷⁷ Ademar, *Chronique*, Bk. III, chap. 64; and Fanning, “Les Origines familiales de Vulgrin,” 253–54. And texts in the cartulary of Vendôme refer to the *fevum* of Saint-Saturnin that Fulk had granted to the *milites* of Goscelinus and Herbrannus, his brother, from Le Mans; *Cartulaire de la Trinité-de-Vendôme*, nos. 66–67.

⁷⁸ For Briollay, see Archives de Maine-et-Loire, H857; and Guillot, *Le Comte d'Anjou et son entourage*, 1: 311, 319, 458. For the date of construction at Matheflon as “peu après 1028,” see *ibid.*, 459; and *Cartulaire du Ronceray*, no. 130. Halphen did not fully appreciate the notion that the grant at Matheflon was made “ab antiquis comitibus,” suggesting both Fulk Nerra and Geoffrey; *Le Comté d'Anjou*, 157–58. It is clear that a stronghold was built at Durtal by Geoffrey Martel sometime after 1047; *Cartulaire de Saint-Aubin*, no. 306. Fulk Nerra’s grandson, however, claimed that his grandfather erected a stronghold at Durtal; Fulk Réchin, *Fragmentum historiae Andegavensis*, 234. It is generally believed that Fulk Réchin erred; Halphen, *Le Comté d'Anjou*, 157. But Halphen had earlier argued that Fulk Nerra began construction at Durtal and Geoffrey Martel completed it; “Étude sur l’authenticité du fragment,” 32–34. It is also possible that Fulk built a stronghold at Durtal, that it was destroyed, and that Geoffrey later rebuilt it; that sequence clearly occurred at Passavant; see note 34, above. La Flèche was fortified sometime before 1060, but, according to Guillot, how much earlier cannot be ascertained; *Le Comte d'Anjou et son entourage*, 1: 461. Le Lude was in Angevin hands no later than 1026, and Isembard of le Lude was part of Fulk Nerra’s entourage prior to 1016; for Fulk’s control of le Lude, see *Annales Vindocinenses*, 61; and, for Isembard, see *Cartulaire de Saint-Aubin*, no. 85. Why Guillot felt it unnecessary to treat this important stronghold is puzzling; *Le Comte d'Anjou et son entourage*, 1: 466 n. 2. On le Lude, also see Halphen, *Le Comté d'Anjou*, 15–16. Hammelinus of Château-du-Loir was a *fidelis* of Fulk Nerra and received from him the churches of Arthezé and Bousse; *Cartulaire de Saint-Aubin*, no. 327. Chartre-sur-le-Loir was in the *mouvance* of the count of Vendôme; L. Denis, ed., *Chartes de Saint-Julien-de-Tours* (Le Mans, 1913), no. 5. A stronghold was built there sometime after 1005. Evidence for a well-established fortification at Chartre-sur-le-Loir comes from a notice that can be dated as having been executed sometime after January 1, 1032, but well before August 15, 1052; *Cartulaire de la Trinité-de-Vendôme*, no. 91; and Guillot, *Le Comte d'Anjou et son entourage*, 2: no. 125. Salomon of Lavardin appears to have been in control of that strategically located site during the reign of Fulk Nerra; Salomon was tied to the Angevin counts at least indirectly through Lancelinus of Beaugency, Viscount Radulf of Saint-Suzanne, and Lisoius of Amboise; *Gesta Ambaziensium Dominorum*, 75–76, 76–77 n. 4. The present rectangular tower that dominates the hill at Lavardin, which overlooks the Loir, was built during the eleventh century and was in place sometime before 1077; Guillot, *Le Comte d'Anjou et son entourage*, 1: 465. Exactly what the fortifications were like at Lavardin earlier in the century and, indeed, perhaps during the tenth century awaits basic archaeological research. Also see Châtelain, *Donjons romans des pays l’Ouest*, 134–35.

distance was sixteen kilometers; the longest leg was twenty kilometers, and the shortest fourteen kilometers. These fortifications provided, in addition, a protected route from Angers to Vendôme north of the Baugé-Morand line.

FULK SEEMS INITIALLY TO HAVE ELECTED to build strongholds to insure his communications with distant possessions and *fideles* against the threat posed by the count of Blois and his supporters. Some of these defended routes—like those in northern Poitou and southwestern Maine—constituted Angevin encroachment on neighbors' lands.⁷⁹ Along with others in the Touraine, these defended routes created a perimeter for an Angevin heartland and thus took on the aspect of a frontier or "limes." Enemy enclaves within this heartland, which had been defined by the "limes," were gradually encircled by Angevin fortifications. Thus the Angevins surrounded Tours and Saint-Aignan and could use Morand as a base camp against Château-Renault. To these can be added Sainte-Maure, which Fulk built ten kilometers east of Odo's stronghold at Île-Bouchard. The perimeter defenses of the heartland that protected Fulk Nerra's communications routes, in combination with the fortifications established to defend important resources (Montbazou to protect Cormery and Montrésor near Villeloin, for example), in effect created a system of "defense in depth" for the Angevin heartland as a whole.⁸⁰

Various groups of strongholds, viewed in relation to significant topographical features, appear to form a strategic unit (see the map, page 535, above). The strength of the diamond formation—Loudun, Moncontour, Mirebeau, and Faye-la-Vineuse—dominating the region between the Dive and the Veude virtually nullified the possibility of a Poitevin attack through that corridor. Similarly, the strongholds at Briollay, Matheflon, and Châteauneuf formed a combination that protected the northern approaches to Angers. Montfaucon, la-Tour-Landry, Maulévrier, Passavant, Vihiers, and Chemillé defended the entire southwest frontier in depth. And, between the Loudun cluster and the Vihiers cluster, the four strongholds of Montreuil-Bellay, Doué, Montsoreau, and Saumur south-southeast of Angers constituted yet another unit.⁸¹

This type of "defense in depth" did not form a rigid, Maginot-like frontier that, when penetrated, results in a debacle.⁸² Rather, the concept is far more subtle, and

⁷⁹ Bachrach, "A Study in Feudal Politics," 120–21.

⁸⁰ For a fine discussion of the notion of "defense in depth," see Luttwak, *Grand Strategy of the Roman Empire*, 132–35. John H. Beeler appreciated the ability of William the Conqueror and his successors to carry out a strategy of "defense in depth" and even used the phrase; Beeler, "Castles and Strategy in Norman and Early Angevin England," *Speculum*, 31 (1956): 581–601. And, although Painter did not use the term "defense in depth," he described such a system on the Scottish border and how it worked, and he discussed "the affection for compact strategic groups of castles" during Stephen's reign, speculating that such an affection had undoubtedly existed much earlier; "English Castles in the Early Middle Ages," 127–28, 132–33. For Sainte-Maure, see Halphen, *Le Comté d'Anjou*, 151, 162.

⁸¹ For Châteauneuf, see *Cartulaire du Ronceray*, no. 235; and, for Doué, which was captured by the Angevins before 1026, see Halphen, *Le Comté d'Anjou*, 40.

⁸² Luttwak, *Grand Strategy of the Roman Empire*, 61.

it was well suited to Fulk's resources, aims, and military organization, particularly given the military potential and aims of his enemies. The most frequent problem that Fulk faced was that of raids such as those carried out by the supporters of Count Odo I, Count Odo II, the viscounts of Thouars, and Count Budic of Nantes.⁸³ These "low intensity" raids were not intended primarily to conquer territory or to capture strongholds but, instead, to acquire booty and, more generally, to undermine the basis of Fulk's legitimacy to rule, at least insofar as it depended on his ability to defend those who recognized his leadership.⁸⁴

In denying easy entrance and exit from the Angevin heartland, Fulk's system of defense in depth effectively thwarted what contemporaries perceived as the goals of his enemies.⁸⁵ Men stationed in towers could not, of course, block the roads or fords by the use of massive fire power in the same manner as defenders of an eighteenth-century fortress could, but archers and men using crossbows could force detours and take a toll among those who were careless enough to come within range of their weapons.⁸⁶ More importantly, however, mounted forces stationed within the strongholds could be deployed outside the walls to harass the enemy, and the horsemen could obtain supplies and camp securely at night within the walls of the various nearby fortifications.⁸⁷ The coordinated use of several such highly mobile forces, as Fulk perhaps envisioned by creating the joint commands under Aimery

⁸³ For the activities of Gelduin of Saumur, see *Historia Sancti Florentii*, 275; and, for those of Odo I's and Odo II's men, see *ibid.*, 281. For the efforts of Abbot Robert's supporters to take control of Meigné, see Archives de Maine-et-Loire, H2191; for the efforts of the viscounts of Thouars and Fulk's worries, see *Cartulaire de Saint-Aubin*, no. 220; and, for the raiding activities of Count Budic, see *Chroniques de Nantes*, chap. 47.

⁸⁴ Luttwak, *Grand Strategy of the Roman Empire*, 66. Throughout the Middle Ages the notion that those holding political authority were responsible for defense was fundamental; for an analysis of this notion with particular reference to the period under discussion here, see Georges Duby, "L'Image du prince en France au début du XI^e siècle," *Cahiers d'histoire*, 17 (1972): 211–16.

⁸⁵ Richer set out Odo's *desiderata*; *Histoire de France*, Bk. IV, chap. 91. For Blésois strategy in the Saumurois, see *Historia Sancti Florentii*, 275–76; and, for Count Budic's aims, see *ibid.*, 283.

⁸⁶ Richer discussed the use of "arcobalistae, sagittarii cum arcubus et balistis" and "sagittae et arcobalistae cum aliis missilibus" at some length; *Histoire de France*, Bk. II, chap. 92, and Bk. III, chaps. 98, 104. J.-F. Finó has defended the accuracy of these texts; Finó, *Fortresses de la France médiévale: Construction, attaque, défense* (3d edn., Paris, 1977), 89. At the famous battle of Hastings, which was won by Fulk's younger contemporary William of Normandy, missile weapons played a key role. The early crossbows were accurate within a range of roughly two hundred meters. S. Toy admitted that the early crossbow was superior to the longbow for the defense of fortifications, but he dated the appearance of the crossbow too late; Toy, *A History of Fortification from 3000 B.C. to A.D. 1700* (London, 1955), 149. For the Gallo-Roman crossbow, see Finó, *Fortresses de la France médiévale*, 46 fig. 7. Vegetius claimed that the effective range of the bow and of the stone-throwing sling was six hundred Roman feet—that is, roughly two hundred yards; *Epitoma Rei Militaris*, Bk. II, chap. 23. Significantly, when Rabanus Maurus drew up his epitome of Vegetius's tract, he acted with the intention of keeping only those things that were of importance "tempore moderno" and made no changes in the text with regard to the bow and sling; E. Dümmler, ed., "De procinctu Romanae Miliciae," *Zeitschrift für deutsches Altertum*, 15 (1872): chap. 13. For archers at Fulk's stronghold of Saint-Florent-le-Vieil, see *Historia Sancti Florentii*, 283. And, for the eighteenth-century comparison, see R. C. Smail, *Crusading Warfare, 1097–1193* (Cambridge, 1956), 204. It seems to me that Painter was too forceful in down-grading the role that strongholds could play in blocking a road or disrupting enemy communications. The fact that the future Louis VIII of France besieged the stronghold at Dover several times suggests that he and his advisers understood the risk of leaving an enemy fortification intact behind their lines; certainly, that siege should not be dismissed, as Painter did, as a case of French efforts to secure their retreat; "English Castles in the Early Middle Ages," 128–29. And, although it is true that medieval armies tried to live off the countryside, Painter asserted too vigorously that they did indeed do so; *ibid.* Basic food supplies that could not be met by foraging had to be transported to the army, and such important matériel as arrows could not be gleaned in this manner from the land.

⁸⁷ For Fulk's "ancient custom" in this regard, see *Cartulaire de Saint-Aubin*, no. 220. Also see note 80, above.

of Vihiers, Roger the Devil, Lisoius of Amboise, and Roger of Loudun,⁸⁸ might well have resulted in the formation of an even stronger system for local defense.

A far less likely threat was a massive invasion of the Angevin heartland by forces intent upon conquering territory or capturing major strongholds. In that event, the many fortifications located in each area were not intended to stop the invasion, merely to impede it.⁸⁹ Any effort to capture one or another fortification provided Fulk with more time to gather a large army with which to meet the invaders, and each stronghold that the enemy bypassed left an intact base, behind the invader's lines, in a position to support a mobile force that could not only harass the march but also cut off supplies and communications.⁹⁰ And any invader who chose to detail separate units to besiege each stronghold that posed a danger risked reducing the size of the main force and leaving the deployed units to sustain themselves in hostile territory against an enemy protected by fortifications. The offensive capacity of Fulk's garrisons against a besieging force was demonstrated at both Montboyau and Saumur when the garrisons sallied forth and burned enemy siege towers.⁹¹

Although it cannot be proved, it is possible—perhaps even probable—that very few major invasions were launched against the Angevin heartland because of the effectiveness, an effectiveness that increased over time as more strongholds were added, of the in-depth defensive system that Fulk had created. Odo II of Blois penetrated as far as Saumur in both 1026 and 1027, but to no avail. The first campaign began with a siege of Montboyau that Odo was forced to abandon when Fulk brought his army to the nearby stronghold of Montbazou and made

⁸⁸ Fulk Nerra built the stronghold at Maulévrier and seems to have placed Aimery as castellan there; Fulk Réchin, *Fragmentum historiae Andegavensis*, 234; Halphen, "Étude sur l'authenticité du fragment," 31; and Bibliothèque Municipale de Poitiers, Coll. D. Fonteneau VI, pp. 623, 629. For further discussion, see Halphen, *Le Comté d'Anjou*, 161 n. 5; and Guillot, *Le Comte d'Anjou et son entourage*, 1: 460. It is clear that, when Aimery was appointed castellan at Maulévrier, he was *oppidanus* of the stronghold at Vihiers; BN, N.A. Lat. 1930, f. 81r. And he appears to have been a member of the garrison at Vihiers prior to becoming a castellan (*oppidanus*); *Cartulaire de Saint-Jouin*, 18. For Roger of Loudun's installation at the newly constructed stronghold at Montrevault, see the charter published in Jacques Boussard, "Les Évêques en Neustrie avant la réforme grégorienne, 950-1050 environ," *Journal des Savants* (1970), 195–96; and, for the identification of Roger, see Guillot, *Le Comte d'Anjou et son entourage*, 1: 240 n. 167. For Lisoius, see *Chronica de gestis consulum Andegavorum*, 48, 54; and *Gesta Ambaziensium Dominorum*, 79, 94. And, for Roger the Devil, see note 39, above.

⁸⁹ Odo I, for example, had to besiege Langeais on two occasions and, when the stronghold held out, was thus unable to penetrate further to the west; Bachrach, "Fortifications and Military Tactics," 114–15 n. 13. For the Blésois attack that was stalled at Amboise, see *Annales Vindocinenses*, 61; and Halphen, *Le Comté d'Anjou*, 44 n. 3; and, for Odo II's efforts to take Montboyau, which caused him a delay and enabled Fulk to gather a large force and counterattack, see *Historia Sancti Florentii*, 265–67. Luttwak's theoretical observations are clearly sustained by the Angevin evidence presented here and by the English evidence Painter developed; see *Grand Strategy of the Roman Empire*, 133; and "English Castles in the Early Middle Ages," 127–28.

⁹⁰ With regard to the medieval situation in general, see J.-F. Verbruggen, *The Art of Warfare in Western Europe during the Middle Ages, from the Eighth Century to 1340*, trans. S. Williard and S. C. M. Southern (Amsterdam, 1977), 290, 293–98. Smail has, however, seen the situation in the Middle East a half-century or so later in a very different light; *Crusading Warfare*, 104–05. Fulk's "ancient custom" and his strategy for the defense of Montreuil-Bellay suggests that the Angevin count appreciated the effectiveness of *caballarii* stationed at a stronghold; see *Cartulaire de Saint-Aubin*, no. 220.

⁹¹ Fulk's garrison at Montboyau successfully executed just such a maneuver; *Annales Vindocinenses*, 60–61; and *Annales qui dicuntur Raimaldi*, 86. The garrison at Saumur was equally successful in this type of operation; *Historia Sancti Florentii*, 280. For a general discussion of the offensive capacity of a fortified garrison with regard to Roman and medieval practice, see Luttwak, *Grand Strategy of the Roman Empire*, 134; and Smail, *Crusading Warfare*, 209.

continuation of the investment impossible. Then, after pursuing Fulk through the southwestern Touraine, Odo advanced to Saumur and in late summer besieged that stronghold. The defenders burned Odo's engines, and, as harvest time approached, the count of Blois had to withdraw because his levies were needed at home to help bring in the crops. The campaign of 1027 began with an unsuccessful siege of Amboise, and Odo changed direction and once again advanced on Saumur. By the time the Blésois forces were ready to deploy, Fulk had brought a substantial army into the area. Odo agreed to negotiate, and the two counts came to an agreement that stipulated that Odo abandon any further effort to take the *castrum*.⁹²

One measure of the effectiveness of Fulk's system is the level of success the Angevin achieved in comparison with that enjoyed by his enemies. The difference is striking. The Angevins and their allies captured the major fortified cities of Nantes and Tours, Poitiers was taken on two separate occasions, and important strongholds—Saumur, Mayenne, Doué, Chinon, Montsoreau, and Gençay—were also captured.⁹³ By comparison, Angevin losses were minimal and temporary, and most of them took place while Fulk was on one of his journeys to the Holy Land.⁹⁴

FULK NERRA'S INITIAL DESIRE TO PRESERVE HIS INHERITANCE led the young Angevin count to pursue a strategy of building strongholds that were in all likelihood intended to secure communications with his dispersed and endangered holdings. The process of building defended lines of communication provided at the same time the "limes" of a greatly expanded Angevin heartland. With lime, sand, and native stone, dozens of fortifications came to extrude from the age-old plain of the

⁹² For the sources, see note 91, above.

⁹³ See Bachrach, "A Study in Feudal Politics," 114–22, and "William the Great," 16; and notes 22 and 74, above. Also see Halphen, *Le Comté d'Anjou*, 39–41. Chinon is a special case; see *Gesta Ambaziensium Dominorum*, 83. Halphen ignored this text and concluded, on the basis of the *Chronica de gestis consulum Andegavorum* that Geoffrey Martel, not Fulk Nerra, gained Chinon for the Angevins. The *Chronica* refers to events in 1044, where concessions are given *de jure* recognition; but this text says nothing about the events of 1037–38. Guillot followed Halphen, with some elaboration, but did not seem to grasp the difference between the *de facto* reality mentioned by the *Gesta Ambaziensium Dominorum* and the *de jure* situation established by the treaty of 1044, which is treated in the *Chronica*. See *Chronica de gestis consulum Andegavorum*, 58; Halphen, *Le Comté d'Anjou*, 48 n. 4; and Guillot, *Le Comte d'Anjou et son entourage*, 1: 63–65, nn. 281, 286, 312. While the stronghold was under Blésois control, the *domini* of Chinon were from the Alo-clan; Werner, "Untersuchungen zur Frühzeit des französischen Fürstentums," 270–77; and Jacques Boussard, "L'Origine des familles seigneuriales dans la région de la Loire moyenne," *Cahiers de la civilisation médiévale*, 5 (1962): 312–14. Before 1040, however, a man named Albericus appears to have held Chinon or, at least, to have served there as castellan. Albericus's son Thomas the *vasvassor* held a *fevum* at la Cour-de-Pierre in the environs of the stronghold at Rochefort-sur-Loire only about sixteen kilometers to the southwest of Angers itself. And Countess Hildegard purchased that *fevum* from Thomas, while Fulk Nerra was still living, to give to Ronceray; *Cartulaire du Ronceray*, nos. 174, 175. Moreover, following the treaty of 1044, which called for both the *de facto* and *de jure* surrender of Château-Renault and of Île-Bouchard by Odo II's *fideles*, there were serious dislocations. But there do not seem to have been problems of dislocation at Chinon following the treaty; Jacques Boussard, "L'Éviction des tenants de Thibaut de Blois par Geoffrey Martel, comte d'Anjou, en 1044," *Le Moyen Âge*, 69 (1963): 144–47. All of this evidence suggests that Chinon passed into Angevin hands before 1044 and under distinctly more favorable terms than those that obtained for the strongholds that surrendered following the implementation of the treaty.

⁹⁴ For example, concerning Langeais, see *Gesta Ambaziensium Dominorum*, 83; concerning Montbazou, see Halphen, *Le Comté d'Anjou*, 42–43; and, concerning Passavant, see note 34, above. Fulk later retook Langeais and Montbazou and rebuilt Passavant, which had been destroyed.

west like so many eruptions of rapidly cooled magma; the landscape that had endured for millennia was changed in the course of a single, event-filled career. No longer was the west a flat land ideal for ranging bands of horsemen. When Fulk had completed his work, the plain was studded with stone peaks that dominated the valleys between them and defined the Angevin heartland in both political and military terms. In the *longue durée*, Fulk's towers, like mountain peaks, gradually returned to dust, but not before the land that they defined and defended served as a base for Geoffrey Plantagenet and his son Henry to secure Maine, dominate Brittany, conquer Normandy, acquire Aquitaine, and humble Blois—and conquer England in the bargain. Fulk Nerra's strategy of castle building provided the foundation upon which his descendants built nothing less than the Angevin empire.

Cobden as Educator: The Free-Trade Internationalism of Eduard Bernstein, 1899–1914

R. A. FLETCHER

BY THE 1890s MANY GERMANS were all too painfully aware that Bismarck's answer to the German question had raised almost as many problems as it had settled. One solution already being touted was that of the irrationalist, antimodernist "conservative revolution." In 1874 Friedrich Nietzsche had published a meditation entitled *Schopenhauer as Educator*. In 1890 a different yet not unrelated sort of cultural pessimist named Julius Langbehn published an extremely popular and influential polemic entitled *Rembrandt as Educator*. In its thirty-ninth edition within two years, Langbehn's passionate, incoherent denunciation of bourgeois values and industrial society called for a cultural regeneration in a wholesale return to the uniquely Germanic values of an idealized past. Following Langbehn's success, a number of books and brochures appeared, offering as "educators," either seriously or satirically, Bismarck, Moses, Moltke, and others. The search for popular mentors persisted at least until 1922, when Martin Havenstein's *Nietzsche as Educator* seemingly completed the circle.¹ All of these formed part of the quest for cultural and spiritual values that lay at the heart of the conservative revolutionaries' attempts to confront the problems of modernity in their German manifestation.

At the other end of the political spectrum, in working-class and socialist circles, a similar debate had long been in progress. By 1890, when the German Social Democratic Party (SPD) emerged from its enforced semi-underground existence, the working-class movement appeared to have decided in favor of a Marxist revolutionary response, which accepted industrialism while aiming at the destruction of bourgeois society. During the 1880s the three individuals most responsible for the triumph of revolutionary Marxism as the dominant and official ideology of the SPD were August Bebel, the party chairman, Karl Kautsky, the premier ideologue of the German party and of the Second International, and Eduard

Research for this paper was sponsored by the German Academic Exchange Service, the Austrian Ministry for Science and Research, and the Commonwealth of Australia Postgraduate Research Fellowship scheme. The author wishes to thank Professor Fritz Fischer of Hamburg for his inspiration and constant encouragement. An earlier version of this paper was presented at the Fifty-Second Congress of the Australian and New Zealand Association for the Advancement of Science, held at Macquarie University, May 1982.

¹ See Fritz Stern, *The Politics of Cultural Despair: A Study in the Rise of the Germanic Ideology* (Berkeley and Los Angeles, 1961), 116. For a more recent, sociologically oriented study of the politics of the radical right in imperial Germany, see Geoff Eley's *Reshaping the German Right: Radical Nationalism and Political Change after Bismarck* (New Haven, 1980).

Bernstein, a former Berlin bank clerk who functioned as editor in chief of the illegal, emigre party newspaper, the *Sozial-Demokrat*. In 1888 Bernstein had been forced to leave Zurich for London, where he remained in exile until his return to Germany in 1901. By the late 1890s, however, Bernstein had ceased to be a revolutionary Marxist and had nailed his colors to the mast as a revisionist—what we now call a democratic socialist. Among Continental socialists, Bernstein's apparent apostasy (his position of trust within the movement was such that Engels had named him as executor of his will) created a furor that was no less strident or intense than that prompted by Langbehn's work. Bernstein's principal attempt to offer a literary justification of his heresies was a book that appeared in 1899 under the title *The Presuppositions of Socialism and the Tasks of Social Democracy* and long remained the "bible of revisionism."²

Bernstein's conversion to a peaceful and gradualist path to socialism without doubt left most of his German comrades bewildered to the point of incomprehension. Typical was the reaction of Lily Braun, a not unsympathetic or uneducated socialist feminist on the right wing of the SPD: on listening to his first public lecture after his return to Germany, her response was a dumbfounded "Was will der Mensch?" ("What on earth is the man driving at?"). The only explanation that made any sense to the vast majority of Bernstein's contemporaries—German and otherwise, friend and foe alike—was that his long years of exile in London had either addled his brain or distorted his vision, leading him to view the world through a Fabian, a Labourite, or a Liberal lens. Such critics were on the right track, although only recently has the lens in question been correctly identified as that of mid- and late-Victorian British radicalism. By considering Bernstein's views on the tariff question, together with his frequently remarked leanings toward neo-Kantianism (still the most commonly alleged source of his indebtedness to liberal ideals), I hope to demonstrate that the thought of Eduard Bernstein—founder and chief ideologue of German and international revisionism, foremost stormy petrel of the Second International, evolutionary socialist and ancestor of the principal socialist alternative to Marxism-Leninism—was in important respects not only fundamentally more British than German but also thoroughly imbued with the values of Cobdenite radicalism.

Three obstacles stand in the way of this enterprise. One is the virtual impossibility of offering an adequate definition of nineteenth-century British radicalism, which was neither a political party nor even a faction so much as a generally moralistic,

² Following the convention of the time, the term "revisionist" is here used (erroneously but conveniently) as a factional label to describe all those elements that made up the right wing of pre-1914 German Social Democracy; the term thus embraces theoretical revisionists like Bernstein, ethical socialists or neo-Kantians like Kurt Eisner, south German reformists like Georg von Vollmar, and, most numerous of all, theoretically indifferent or self-consciously atheoretical party practitioners like Ignaz Auer, Gustav Noske, and Friedrich Ebert as well as imperialists like Joseph Bloch. On Bernstein, see Peter Gay, *The Dilemma of Democratic Socialism: Eduard Bernstein's Challenge to Marx* (Collier edn., New York, 1962); Pierre Angel, *Edouard Bernstein et l'évolution du socialisme allemand* (Paris, 1962); Bo Gustafsson, *Marxismus und Revisionismus: Eduard Bernsteins Kritik des Marxismus und ihre ideengeschichtlichen Voraussetzungen*, trans. Holger Heide (Frankfurt, 1972); Thomas Meyer, *Bernsteins konstruktiver Sozialismus* (Berlin, 1977); Horst Heimann and Thomas Meyer, eds., *Bernstein und der demokratische Sozialismus* (Berlin, 1978); and Herbert Frei, *Fabianismus und Bernsteinscher Revisionismus, 1884–1900* (Bern, 1979).

often patriotic, and pre-eminently critical frame of mind deploring the lack of principle and ideas in politics. Even Cobden and Bright were not always of one mind, and there were many issues on which Victorian and Edwardian radicals remained deeply divided, a notable example being the fissure between noninterventionists and liberationists.³ A second difficulty arises from Bernstein's frequent denials of the charge that he contemplated the world through an English monocle. Like E. D. Morel in Britain (after 1908 this somewhat naive but highly influential radical publicist, later prominent in the Union of Democratic Control and as an architect of Labour Party foreign policy, worked to avoid war by appeasing Germany and destroying the Foreign Office "dictatorship" over British foreign policy), Bernstein was widely condemned as being nothing less than a foreign agent; his detractors ranged from patriotic Reichstag deputies to fellow revisionists.⁴ If he hoped to retain any political effectiveness in Wilhelmine Germany, Bernstein had no choice but to play down his Anglophilia and his indebtedness to British models. Here his denials are thus discounted as being essentially the product of political expediency. Finally, it must be admitted, intellectual influence is extremely difficult to prove. I could point to Bernstein's British contacts, to the judgments of his contemporaries, and to the results of recent scholarship,⁵ but the method I have followed here is to allow Bernstein to speak for himself, for the internal evidence from his own writings is sufficiently compelling to suggest the existence of strong parallels between Bernstein and the stock in trade of Cobdenite radicalism.

ON QUESTIONS OF TARIFF POLICY—more than in his attitudes toward the problems of nationality, militarism, or even imperialism—Bernstein's unequivocal condemnation of protectionism and his sweeping endorsement of free trade treated Marx as an irrelevancy to be bypassed and ignored. Marx and Engels contemplated tariff policy almost exclusively from the standpoint of its probable bearing on the revolution, inclining strongly to free trade precisely because it seemed to them to be natural to capitalism and most likely to effect the earliest and full maturation of the capitalist system.⁶ Since Bernstein had long ceased to regard socialist revolution as practicable or even desirable, and since he had also had to consider the question in the context of imperialism, a force that Marx had not foreseen, that Bernstein and Marx were at cross purposes on this issue is hardly surprising. In matters of tariff

³ See John W. Derry, *The Radical Tradition* (London, 1967), vii–xi; A. J. P. Taylor, *The Trouble Makers: Dissent over Foreign Policy, 1792–1939* (London, 1957), 11–24; Michael Howard, *War and the Liberal Conscience* (London, 1978), 46–72; Keith Robbins, *John Bright* (London, 1979), 101–02, 107, 137, 171; and D. W. Bebbington, *The Nonconformist Conscience: Chapel and Politics, 1870–1914* (London, 1982), 106–26.

⁴ See Catherine Cline, *E. D. Morel, 1873–1924: The Strategies of Protest* (Belfast, 1980), 68–97; and Francis L. Carsten, *War against War: British and German Radical Movements in the First World War* (London, 1982), 30–31, 176–77.

⁵ I have attempted to explore these analytical approaches in my "Bernstein in Britain: Revisionism and Foreign Affairs," *International History Review*, 1 (1979): 349–75.

⁶ Hans-Christoph Schröder, *Sozialismus und Imperialismus: Die Auseinandersetzung der deutschen Sozialdemokratie mit dem Imperialismusproblem und der "Weltpolitik" vor 1914* (2d rev. edn., Bonn, 1975), 48, 79–89; Shlomo Avineri, *The Social and Political Thought of Karl Marx* (Cambridge, 1968), 252; and Tom Kemp, *Theories of Imperialism* (London, 1967), 8–29.

policy Bernstein sought inspiration, therefore, not from Marx but—like Marx himself—from classical economic liberalism. Generally much more explicit in opposing protectionism than in expounding free trade, a doctrine he approached more from the standpoint of international trade than from that of national economics, Bernstein consistently revealed a perspective that was, on the whole, less economic than political and ethical, as indeed Richard Cobden's had been.⁷

On socioeconomic grounds Bernstein condemned protectionism as inherently reactionary ("a crude expedient, inherited from a time when scientific administration was practically non-existent"), unrelated to capitalism, and utterly indefensible. That it had found favor among the bourgeois parties in Germany and elsewhere merely registered the increasingly reactionary stance of these parties, which had lost the courage "to stand up for their former ideals."⁸ That its introduction coincided with a great upsurge in German trade and industry was no argument for protection: free-trade economies had also prospered and German prosperity might have been greater still under a free-trade system, since the recent growth of the German economy had largely been due to the achievement of national unity, technical progress, population increase, and the great mineral wealth of Germany. Protectionism could be granted some measure of economic validity only during the initial take-off stage of industrialization, but he confessed to skepticism even in regard to the theory of educational duties or teething tariffs that Friedrich List had advocated.⁹ The British experience had, Bernstein believed, demonstrated beyond doubt that free trade was superior to protectionism at all stages of economic development.¹⁰

In essence, protectionism meant agrarian protectionism, for German industry generally had no need of protective tariffs. But neither agriculture nor industry stood to gain from this system. Economically, it was of no use to agriculture; protectionism did not increase grain production, it did not effectively counter foreign competition, it did not increase agricultural profits, and it did not promote agrarian rationalization. By inflating rents, interest rates, and prices, protectionism functioned as a crisis-producing doctrine of the first magnitude. Although protec-

⁷ F. H. Hinsley, *Power and the Pursuit of Peace: Theory and Practice in the History of Relations between States* (Cambridge, 1963), 96–97; Bernard Semmel, *The Rise of Free Trade Imperialism* (Cambridge, 1970), 162–63; and John A. Hobson, *Richard Cobden: The International Man* (new edn., London, 1968), 36–43. On the development of Cobdenite radicalism to 1914, see the important articles by Peter Cain: "Capitalism, War, and Internationalism in the Thought of Richard Cobden," *British Journal of International Studies*, 5 (1979): 229–47, "International Trade and Development in the Work of J. A. Hobson before 1914," *History of Political Economy*, 11 (1979): 406–24, and "J. A. Hobson, Cobdenism, and the Radical Theory of Economic Imperialism, 1898–1914," *Economic History Review*, 31 (1978): 565–84.

⁸ Bernstein, "German Professors and Protectionism," *Contemporary Review*, 86 (1904): 31, and *Protokoll über die Verhandlungen des Parteitag der Sozialdemokratischen Partei Deutschlands, abgehalten zu Bremen, 1904* [hereafter, *Bremen Protokoll*] (Berlin, 1904), 229. Also see his "Deutschland als Konkurrent Englands," *Die Neue Zeit* [hereafter, *NZ*], 14 (1895–96), 2: 757–58. Bernstein demonstrated protectionism's irrelevance to capitalism by contrasting Britain as "the bastion of free trade" with Russia as "the bulwark of protective tariffs and monopolies"; Bernstein, "Kreta und russische Gefahr," *NZ*, 15 (1896–97): 17.

⁹ Bernstein, "The Growth of German Exports," *Contemporary Review*, 84 (1903): 785–86, "Zum Kampf gegen die Zollschrabe," *Sozialistische Monatshefte* [hereafter, *SM*] (1901), 2: 688, "Was treibt England zum Reichszollverein?" *ibid.*, (1904), 2: 549, and "German Professors and Protectionism," 18–20.

¹⁰ Bernstein, "Zum Kampf gegen die Zollschrabe," 689, "Englands Wirtschaftsentwicklung im letzten Jahrzehnt," *SM* (1904), 2: 813, "Die internationale Politik der Sozialdemokratie," *ibid.* (1909), 2: 621, and *Die englische Gefahr und das deutsche Volk* (Berlin, 1911), 40.

tionism served a legitimate aim in that it professed to enhance the competitiveness of domestic grain production, the best that could be said of it was that it was the wrong remedy. Bernstein denied that protective tariffs bestowed any worthwhile benefits on industry and trade. The alleged advantages of protectionism (such as those stemming from cartellization and dumping) could not apply equally to all branches of industry. Indeed, some trades could not benefit at all from protection, and quite a few were even harmed by its apparently successful application in others. Bernstein was particularly critical of the benefits that protectionism allegedly accorded to export industry, whose unlimited expansion he in any case declined to accept as an unqualified blessing under all circumstances. For Germany's strongest export industries were either unprotected or more harmed than helped by protective duties. Citing recent trade statistics, he argued that the greatest increase in German exports had taken place under the Caprivi system, when relatively free trade had prevailed, and in the face of economic recession.¹¹

If protectionism offered any substantive beneficial export incentives, their value was strictly limited by the necessity of maintaining a fine balance between foreign and domestic sales. The more one exported at dumping prices, in other words, the more one was obliged to sell or raise prices on a domestic market that, even under monopolistic conditions, was not infinitely elastic. In any event, such advantages, whether real or imagined, were largely irrelevant, for competitiveness on the world market was determined above all by "technical science, [which] knows so far neither protective duties nor duties on exports." Moreover, Bernstein argued, protectionism entailed clearly identifiable and serious economic disadvantages. Not only did it create an unfavorable trade balance but it also fostered "monopolistic positions of the worst sort" and a high-cost economy. It strained domestic consumption to the utmost, subsidized inefficiency, and prevented industrial rationalization.¹²

Although it might help some people brought to the point of economic ruin by foreign competition, protectionism "always enriches people who need no help at all, at the cost of the masses who are grievously needy." Not only did protectionism bring higher prices, lower living standards, and fewer employment opportunities for the masses, but it also, by so burdening the working classes and restricting consumer purchasing power, simultaneously transferred resources from the legitimate satisfaction of broad human needs to the indulgence of minority whims and fancies. To this extent it was socially as well as economically objectionable, indeed antiprogressive (*fortschrittsfeindlich*).¹³ By "breeding capitalists at the expense of the masses" and artificially prolonging the superfluous and parasitic existence of uneconomic agrarians, protectionism in addition enabled "a reactionary social

¹¹ Bernstein, "Das Grundsätzliche in der Frage der Handelspolitik," *SM* (1911), 1: 429–30, "Prinzipielles zur Frage der Agrarzölle," *ibid.* (1901), 1: 189–90, "Zollfreier internationaler Verkehr," *ibid.* (1911), 2: 831, and "The Growth of German Exports," 780–82.

¹² Bernstein, "The Growth of German Exports," 782, 777, 783–87, "Prinzipielles zur Frage der Agrarzölle," 190, *Die englische Gefahr*, 41, "Das Grundsätzliche in der Frage der Handelspolitik," 430, and *Die neuen Reichsteuern* (Berlin, 1906), 62.

¹³ Bernstein, "German Professors and Protectionism," 31, "Prinzipielles zur Frage der Agrarzölle," 189–90, "The Growth of German Exports," 777, 780, *Die neuen Reichsteuern*, 62, *Stenographische Berichte über die Verhandlungen des Deutschen Reichstages* [hereafter, *Verhandlungen DR*], December 11, 1905, p. 223, and "Zum Kampf gegen die Zollschrabe," 687.

stratum to maintain its way of life and give effective representation to its political tendencies." To Bernstein it seemed almost as though the infamous Bülow tariff of 1902 had been specifically designed to encapsulate in concentrated form all of these social and economic evils.¹⁴

Since it could delay but not prevent the economic development of Germany, protectionism represented more a political than an economic menace. It had been fetched by Bismarck from the rubbish-heap of history not merely to shore up the tottering finances of a handful of his fellow Junkers but above all to preserve indefinitely an antiquated and moribund semi-feudal social order. In origin and nature German protectionism was animated fundamentally by "the political rather than the economical point of view, home policy rather than foreign." Since Bismarck, its proponents had appealed to a variety of specious but spurious arguments, such that not even a Listian national economist could in conscience endorse, to thrust on the German nation an autarkic economic policy that aimed primarily at insulating Germany from the world economy.¹⁵

Bernstein deplored this objective as "a utopia, and by no means an attractive utopia," for it was as undesirable as it was unattainable. Germany, like all great trading nations, had certainly become dependent on foreign markets and sources of supply, but this was "not at all an unmitigated evil" in that "mutual dependence of countries on one another" assured general prosperity, peace, and progress in civilization.¹⁶ The autarkist arguments advanced by economists like Albert Schäffle, Karl Oldenberg, and Adolf Wagner were in fact "sheer romanticism." Out of political necessity they were based on hypothetical dangers that were, by their authors' own admission, highly improbable. This same political necessity had obliged Bismarck to extend protectionism to heavy industry and to persuade the bourgeois parties of the existence of a general threat to national prosperity and social order. Protectionism, which was "impossible to defend . . . by true economic arguments," was nothing if not a weapon designed to stem the social and political tide of industrialism by attacking democracy and socialism within Germany.¹⁷

The danger in this mercantilist atavism, in Bernstein's view, lay mainly in the serious obstacles it raised to domestic social and political reform. Foreign countries had nothing to fear from German protectionism unless they neglected public education, overextended themselves in imperial ventures, or also adopted protec-

¹⁴ *Neue Hamburger Zeitung*, October 31, 1901; Bernstein, "Prinzipielles zur Frage der Agrarzölle," 189, "Zum Kampf gegen die Zollschraube," 687, and "The Growth of German Exports," 787; and Eduard Bernstein, ed., *Dokumente des Sozialismus* [hereafter, *DdS*], 2-3 (1903): 392.

¹⁵ Bernstein, "German Professors and Protectionism," 27, 21, 25-27, 30, and *Die englische Gefahr*, 7. Bernstein believed that the views of conservative economists like Wagner, "if consistently acted upon, would lead to a Chinese state of civilization" and "aggravate the evils pointed out"; "German Professors and Protectionism," 25.

¹⁶ Bernstein, "Prinzipielles zur Frage der Agrarzölle," 189, "Zum Kampf gegen die Zollschraube," 687-88, "The Growth of German Exports," 777, "German Professors and Protectionism," 20, 25, and "Allerhand moderner Spuk," *SM* (1912), 1: 344.

¹⁷ Bernstein, "German Professors and Protectionism," 30, 21-31. On these men and their social and political views, see K. D. Barkin, *The Controversy over German Industrialization, 1890-1902* (Chicago, 1970), 131-85; and, on the social and political function of protectionism in Germany, see H. A. Winkler, ed., *Organisierter Kapitalismus* (Göttingen, 1974); Hans-Ulrich Wehler, *Das Deutsche Kaiserreich, 1871-1918* (Göttingen, 1973); and Martin Kitchen, *The Political Economy of Germany, 1815-1914* (London, 1978).

tionism.¹⁸ In the external environment protectionism might be inherently destructive, but it was not necessarily expansionist. Canada, for instance, discriminated heavily against British goods, but this was so far removed from any hostile political intent that in the event of an Anglo-German war Canada, like the whole of the English-speaking world, would almost certainly, and willingly, offer its wealth and its manhood in defense of Britain. In Germany, to be sure, protectionism had become part and parcel of a "system" of mutually supportive evils—militarism, navalism, colonialism, official religion, the Prussian class suffrage, residual feudal monarchism, and German federalism. This system stood "in all possible spheres in contradiction . . . to the great stream of natural evolutionary tendencies in modern international life."¹⁹ The threat, however, was more psychological than real. Feeding on an atmosphere charged with fear and hostility, protectionism tended to create the very perils it purported to counteract. Although he admitted there was some danger of "destructive wars" in "the tendency of German chauvinists to imitate 'now that we are great' the faults of other nations," Bernstein saw this menace arising principally from misguided assumptions of political rivalry and animosity tending to endow unreal fears and imaginings with tangible form, so that in this sense, too, protectionism functioned "like drowning a man to save him from the shock of a dip."²⁰

In the world economy protectionism represented a retarding factor, but it could not significantly impede the internationalization of trade and business on which the progress of humanity and the civilization of the globe so heavily depended. Despite the wave of protectionism that had engulfed the Continent and North America, free-trade Britain continued to prosper and even to expand its share of world trade.²¹ Politically, however, the question had another, more threatening aspect. What Bernstein disliked most about protectionism was less its "false economic theories" *per se* than their destructive implications for relations among civilized states, especially for Anglo-German relations. He argued that this "perverse mode of economic thought" was tending toward the breakdown of economic internationalism and creating in its place artificial conflicts among nations. It confused the minds of whole peoples on the great issues facing humanity and supplanted the "uplifting sense of cultural community and the creative cooperation it produced" with a "stupidly barbarous mistrust among neighboring peoples" and "an insane superstition" regarding the true interests of nations.²² Here, as elsewhere, Bernstein customarily identified Bismarck as the principal malefactor, but a large measure of responsibility also had to be borne, in Bernstein's view, by those

¹⁸ Bernstein, "The Growth of German Exports," 787. For the Cobdenite core of this view, compare Donald Read, *Cobden and Bright: A Victorian Political Partnership* (London, 1967), 110.

¹⁹ Bernstein, *Die neuen Reichssteuern*, 63–64, "Was treibt England zum Reichszollverein?" 536, *Die englische Gefahr*, 45, "Neue Englandhetze," *Vorwärts*, September 1, 1911, and "Der kanadisch-amerikanische Schicksalsschlag," *SM* (1911), 1: 301–11.

²⁰ Bernstein, "German Professors and Protectionism," 23, 30, 25, 29, and "The Growth of German Exports," 787.

²¹ Bernstein, "Zum Kampf gegen die Zollschraube," 190, "Die internationale Politik der Sozialdemokratie," 621, and *Die englische Gefahr*, 39–40.

²² Bernstein, *Die englische Gefahr*, 41.

economists who persistently interpreted Friedrich List in a one-sided, class-interested manner. The neo-Listians and the interest groups for whom they spoke had not merely distorted the teachings of their mentor by misusing his advocacy of temporary and selective teething tariffs to elevate neo-mercantilist protectionism to an absolute principle; just as they ignored List's view that "international free trade was the commercial system to be aimed at," they passed over in silence his enthusiastic admiration of the English model to misappropriate his "abusive attacks on perfidious England" on behalf of a "stupid Anglophobia," which they misrepresented to the German people as a product of economic necessity.²³ Insofar as this protectionism was also "the foster-mother of navalism and the colonial fever," socialists were obliged by their commitment to peace and internationalism to oppose it and to campaign for freedom of international commerce.²⁴

BERNSTEIN'S DEFENSE OF FREE TRADE was more sketchy. Indeed, he once stated his belief that free trade needed no defense because its pacifist tendency was self-evident. Yet he did present a Cobdenite defense of what he preferred to call "duty-free international intercourse." A key factor in this defense was his endeavor to demonstrate empirically that free trade was the best available means of promoting international trade and general prosperity.²⁵ In bringing peoples together in mutually profitable commercial relations, free trade simultaneously created a host of common interests, bonds, and interdependent relationships of an intellectual and spiritual as well as material kind—in law, learning, art, politics, and so on. In time, these relationships, spanning the whole range of human experience and activities, would naturally become institutionalized. By fostering internationalism through prosperity,²⁶ "duty-free international intercourse" enhanced the spread of European culture, enlarged the area of human freedom, encouraged international understanding, and created a general interest in peace. As such, it functioned as an agent of social progress in the broadest and most meaningful sense.²⁷ By contrast,

²³ Bernstein, *Die neuen Reichssteuern*, 4–5, *Die englische Gefahr*, 5, 7, "German Professors and Protectionism," 18–22, and "Breakers Ahead," *The Nation*, September 2, 1911, p. 804.

²⁴ Bernstein, *Die neuen Reichssteuern*, 64, and "Das Grundsätzliche in der Frage der Handelspolitik," 428. Even after the Agadir crisis he professed to believe that the influence of the protectionist, imperialistic, and reactionary heavy industry lobby in Germany was "on the wane"; "Breakers Ahead," 804. And, although he recognized a reciprocal connection between such forces, only after the outbreak of the Great War did he acknowledge a causal or necessary connection between protectionism and imperialism, as, for example, in his description of imperialism as "to a high degree the product . . . of protectionist reaction"; Bernstein, *Sozialdemokratische Völkerpolitik* (Leipzig, 1917), 114.

²⁵ Bernstein, *Sozialdemokratische Völkerpolitik*, 170. Thus he compared the trade and prosperity of the Australian colonies of Victoria (protectionist) and New South Wales (free trade) to demonstrate that the economy of the latter was in a much more flourishing condition; "Zum Kampf gegen die Zollschraube," 689.

²⁶ For a characteristically Cobdenite statement of this view, linking free trade with peace, retrenchment, and reform, see Bernstein, *Die neuen Reichssteuern*, 10.

²⁷ See Bernstein, "The Zeppelin Movement and German Nationalism," *The Nation*, August 22, 1908, p. 737, "Peace and King Edward's Visit," *ibid.*, February 20, 1909, 784, *Die englische Gefahr*, 84, "Das Grundsätzliche in der Frage der Handelspolitik," 430, *Sozialdemokratische Völkerpolitik*, 172–74, 177, *Völkerbund oder Staatenbund* (Berlin, 1918), 28, *Völkerrecht und Völkerpolitik* (Berlin, 1919), 184–85, "Zum Kampf gegen die Zollschraube," 687 n., *Verhandlungen DR*, December 12, 1903, p. 108, and "Das Finanzkapital und die Handelspolitik," *SM* (1911), 2: 955; and Haase, *Chemnitz Protokoll* (1912), 420. Although, like Cobden, Bernstein cherished free trade not just as an economic end in itself but also for its presumed contribution to such larger political ends as

protectionism, which had an exclusively national application, did not even pretend to serve such progressive ends.

But free trade also offered national advantages. Bernstein denied that it was antinational and that it impaired the development of relative latecomers to industrialization, like Germany. Friedrich List had argued, "to some extent quite rightly," that "it would be the greatest injustice to proclaim the existing division of labour as the proper state of things and so confine the great mass of the German nation to mere agricultural pursuits for an indefinite length of time." Britain's early industrial monopoly as the "workshop of the world" was thus conflated with its then existing industrial supremacy, which Germany was adjured to resist, and such resistance warranted state intervention, for, "in principle, state encouragement to the competitiveness of domestic production is thoroughly justifiable." Seeking, yet again, to have his cake and eat it too, Bernstein here supplied an *international* vindication of state intervention on behalf of a sound national economy: "As firmly as I believe in the league of peoples, just as little do I believe in a dissolution of nations in the foreseeable future. . . . In my view, the great republic of peoples imposes a duty to national health. But it can be satisfied otherwise than by means of customs duties."²⁸ Whatever these unspecified means might be, they must not encroach on duty-free international intercourse.

Bernstein's other counter to the interpretation of free trade as a weapon of British imperialism—Marx's view, later repeated by Lenin and Trotsky to explain the corruption of the European proletariat by imperialist superprofits, was already being manipulated, as Bernstein was painfully aware, to demand socialist support for German imperialism—was the rather lame argument that "neither Cobden nor Bright nourished the utopian thought that England could by any means—free trade or other—prevent the industrial progress of other nations." Yet he could not deny that British exports to the Continent in the wake of the Napoleonic wars had had a devastating effect on nascent German industries.²⁹ In short, by his failure to distinguish clearly between *laissez faire* and free trade, and by his inability effectively to dispose of the argument that free trade tended to perpetuate Britain's industrial supremacy, Bernstein necessarily presented a confused and already prejudiced case for free trade as a dictate of German national interest.

His case ran as follows. Free trade favored export industries while also promoting greater efficiency through international specialization. At the same time it lowered

peace and the partnership of Western humanity, this by no means implied a commitment to peace at any price, any more than it had done with Cobden, John Stuart Mill, and others. See Taylor, *The Trouble Makers*, 126; Hinsley, *Power and the Pursuit of Peace*, 96; Norman Angell, *After All* (London, 1951), 169; and E. K. Bramsted and K. J. Melhuish, *Western Liberalism: A History in Documents from Locke to Croce* (London, 1978), 285–87.

²⁸ Bernstein, "German Professors and Protectionism," 22–23, "Zollfreier internationaler Verkehr," 831, and "Das Grundsätzliche in der Frage der Handelspolitik," 428–29.

²⁹ Bernstein, "The Zeppelin Movement and German Nationalism," 737, and "German Professors and Protectionism," 19. In 1901, Bernstein attributed such a position to English free-trade theorists generally; "Zum Kampf gegen die Zollschrabe," 688–89. That there was in fact a markedly imperialist element to the thinking of many Victorian free-trade partisans now seems beyond dispute. See Semmel, *The Rise of Free Trade Imperialism*; K. Bourne, *The Foreign Policy of Victorian England, 1830–1902* (Oxford, 1970); B. Porter, *The Lion's Share: A Short History of British Imperialism, 1850–1970* (London, 1975); P. J. Cain, *Economic Foundations of British Overseas Expansion, 1815–1914* (London, 1980); and W. G. Hynes, *The Economics of Empire* (London, 1979).

production costs by reducing raw material and foodstuff prices.³⁰ The removal of agrarian duties in particular would, by reducing living costs, stimulate consumer spending and domestic consumption while also promoting agricultural rationalization and modernization. The Reich could recoup the revenue thereby forfeited through the imposition of modest direct taxes on income and inheritances, taxes that would not only enable the Reich government to meet its existing financial commitments but also relieve the poorer states of the sometimes crippling burden of matricular contributions and provide a surplus adequate to cover the additional costs of necessary social reforms.³¹ Freer movement of labor need not result in the social dislocations, lower wages, and depressed living standards that might accompany a sudden influx of cheap foreign labor—"coolies and similar inferior workers" (*Kulis und dergleichen tiefstehenden Arbeitern*). That problem could easily be regulated through immigration laws and social policy.³²

But was there a specifically socialist case to be made for free trade? Bernstein claimed that there was, although he evidently had some difficulty locating its components. He described free trade as being, together with a democratic finance policy, a "self-preservation commandment of the common weal" (*Selbsterhaltungsbefehl der Allgemeinheit*) and even maintained that it transcended class interest.³³ A point he frequently stressed was the desirability of free trade as required by the "cultural mission of the working class" and its commitment to progress,³⁴ a point he bolstered by reassurances about the political viability of free trade. Reactionary as the nonsocialist parties might be, he never tired of reminding his readers that significant bourgeois elements, in Germany and in general, had a vital interest, however dimly perceived, in the preservation or restoration of free trade.³⁵ Among these elements he specifically included "the banks, which are interested in free trade in capital."³⁶ What the working class stood to gain from free trade was a larger slice of a growing national cake, for free trade meant a redistribution of income in favor of export industry, which in Germany already employed a large majority of

³⁰ Bernstein, "The Growth of German Exports," 777–80, "Das Grundsätzliche in der Frage der Handelspolitik," 430, and "Zollfreier internationaler Verkehr," 828–29.

³¹ Bernstein, "Die Entwicklung der Agrarverhältnisse in England" (1897), in *Zur Theorie und Geschichte des Sozialismus*, 3 vols. (4th edn., Berlin, 1904), 1: 41–57, "Prinzipielles zur Frage der Agrarzölle," 190, *Die neuen Reichssteuern*, 56–64, and "Das Grundsätzliche in der Frage der Handelspolitik," 430.

³² Bernstein, "Zum Kampf gegen die Zollschrabe," 693.

³³ Bernstein, *Die neuen Reichssteuern*, 63, and *Sozialdemokratische Völkerpolitik*, 173.

³⁴ Bernstein, "Zollfreier internationaler Verkehr," 832. Also see Bernstein, "Zum Kampf gegen die Zollschrabe," 692, *DdS*, 1 (1902): 282, and "Das Grundsätzliche in der Frage der Handelspolitik," 428.

³⁵ Bernstein, "Zur Bilanz der Kämpfe gegen den neuen Zolltarif," *SM* (1903), 2: 38–41, "Arms and the Bill," *The Nation*, March 20, 1909, p. 929. "Das Finanzkapital und die Handelspolitik," 952, 955, and "Politische Schwarzmalerei," *SM* (1912), 1: 538–44. The last two articles listed were polemics directed explicitly at Hilferding and Otto Bauer.

³⁶ Bernstein, "Das Grundsätzliche in der Frage der Handelspolitik," 425. Thus he was at odds not only with the neo-Marxist position of Hilferding, Bauer, Parvus, and Luxemburg but also with that of British radicalism represented by Hobson and Brailsford. On the latter, see J. A. Hobson, *Imperialism: A Study* (rev. edn., London, 1938), 71–93; H. N. Brailsford, *The War of Steel and Gold* (London, 1914), 63–86, 232–35; Taylor, *The Trouble Makers*, 95–131, esp. 100–01, 122–23; Z. S. Steiner, *Britain and the Origins of the First World War* (London, 1977), 146–47; and A. J. A. Morris, *Radicalism against War, 1906–1914* (London, 1972). Without naming Hobson as its true author (Hobson first stated his thesis in two articles published in the *Contemporary Review* in 1898), Bernstein branded the neo-Marxist theory a derivative of economic liberalism; *Zur Theorie und Geschichte des Sozialismus*, 2: 110.

the industrial workforce in a steadily expanding national economy. Free trade therefore meant more jobs at higher wages, shorter hours, and lower living costs.³⁷ Over and above such gains, the prosperity that it generated was generally beneficial to the working class in that "the richer the society, the more easily and surely socialist gains can be realized."³⁸

In a rare speculation as to the probable tariff policy of the *Zukunftsstaat*, he further argued in 1911 that a socialist state would necessarily favor free trade because a state that had socialized exchange would be taxing no one but itself if it imposed protective tariffs.³⁹ Given Bernstein's view of socialism as a mere ideal worth striving toward, this palpably unconvincing piece of sophistry may best be seen as a nervous, indirect tribute to the success with which the protectionist writings of fellow revisionists like Max Schippel, Richard Calwer, and Gerhard Hildebrand were already meeting among large sections of the Social Democratic rank and file.

In his position on the tariff question, Bernstein revealed himself, even more starkly than elsewhere, as having been thoroughly imbued with the Cobdenite tradition of British radicalism. Here he hardly ever referred to Marx. When he appealed to authority, the names he mentioned were far more likely to be Jeremy Bentham, Richard Cobden, John Bright, William E. Gladstone, John Stuart Mill, or John Prince Smith. In the wider sense, Bernstein belonged to this tradition. Much as he disliked being labeled an advocate of Manchesterism, there can be little doubt that he knew and reflected all the principal ideas of this school. The Benthamite-Cobdenite school held that, whereas conflict prevailed between states, there existed a natural harmony among peoples and nations; that free trade and peace were "one and the same cause"; that the advancement of freedom and civilization were better served by peace, trade, and public education than by government; that diplomacy, war, and the balance of power were outmoded as well as wicked and inimical to human happiness; that arbitration, disarmament, and nonintervention were desirable goals; that free trade and peace would eventually bring world government; that intervention was permissible only in support of liberty; and that barbarians were incapable of either independence or nationality. If Bernstein was not always completely in accord with these views, his digressions and qualifications no more took him beyond the fold of the "dissenting" tradition than did those of John Stuart Mill.

In some respects Bernstein was, in fact, more an unreconstructed Cobdenite than were many of his English radical contemporaries. The innovations introduced by J. A. Hobson, for example, presented a hurdle that Bernstein refused to jump at, in part because of his strong attraction to the Fabian version of the national-efficiency

³⁷ Bernstein, "Das Finanzkapital und die Handelspolitik," 955, and "Zollfreier internationaler Verkehr," 828–29.

³⁸ Bernstein, "Die Zusammenbruchstheorie und die Kolonialpolitik," *NZ*, 16 (1897–98), 1: 556. Bernstein's position was therefore not radically different in principle from that of Joseph Bloch, Karl Leuthner, Max Schippel, and company. On the Blochian position, see R. A. Fletcher, "Revisionism and Empire: Joseph Bloch, the *Sozialistische Monatshefte* and German Nationalism, 1907–14," *European Studies Review*, 10 (1980): 459–84.

³⁹ Bernstein, "Das Grundsätzliche in der Frage der Handelspolitik," 426.

point of view.⁴⁰ When he was in Britain, the Fabians had been free-traders. By 1903 they had come out in favor of protectionism. George Bernard Shaw, certainly, was a “thorough protectionist” and agreed with Thomas Carlyle that free trade was “heartbreaking nonsense.”⁴¹ Bernstein also balked at this fence. Although he learned a great deal from the British bourgeois radical tradition (principally through such intermediaries as the Fabians and the ethical socialists) and drew inspiration from the whole range of its offshoots and successor movements, from “Dissenters” both old and new, from the “Limps” (Liberal Imperialists like Rosebery and Haldane) and even to some extent from members of the fundamentally illiberal “national efficiency group” (the Webbs, for example),⁴² Bernstein remained at heart firmly committed to the Cobden-Angellite radical orthodoxy. This commitment, first entered into in the mid-1890s,⁴³ intensified after his return to Germany and reached its acme in his post-1914 antiwar radicalism.

AMONG POST-BISMARCKIAN GERMAN LIBERALS, as Bernstein well knew, support for free-trade principles had become as rare and as unpopular as a sincere and steady commitment to democratic principles. Such support was confined to a few intellectual mavericks like Lujo Brentano, to the left liberal splinter parties, and to interest groups like the *Hansabund*. Yet even the *Hansabund* in his view represented little more than a revolt by export industry against the economic burdens of exorbitant protectionism; certainly it was not a force for a more liberal foreign

⁴⁰ His main quarrel with Hobsonian radicalism was, of course, his optimistic interpretation of the future of capitalism in general and of the role of finance capital in particular. Bernstein could not accept Hobson's view without jeopardizing the central thesis of his revisionism, which asserted that advanced capitalism, far from heading toward greater and more frequent crises culminating in a general collapse, was growing more flexible, stable, and prosperous, that this process was significantly aided by novel developments such as the progressive, ameliorative function of the modern credit system, and that the working class, through democracy, was at last in a position to effect cumulative improvements in its situation to the point of compelling a gradual transformation toward socialism. See Bernstein, *Die Voraussetzungen des Sozialismus und die Aufgaben der Sozialdemokratie* (Stuttgart, 1899), and its English translation, *Evolutionary Socialism*, trans. Edith Harvey (London, 1909). The English edition does not contain the key chapter on Marx and the Hegelian dialectic.

⁴¹ Shaw, “The Solidarity of Social Democracy” (1906), in L. J. Hubenka, ed., *Practical Politics* (Lincoln, Neb., 1972), 14. For the Fabian position on the tariff question, see A. M. McBriar, *Fabian Socialism and English Politics, 1884–1914* (Cambridge, 1962), 131–34.

⁴² G. R. Searle, *The Quest for National Efficiency, 1899–1914* (Berkeley and Los Angeles, 1971), 54–101; Günter Hollenberg, *Englisches Interesse am Kaiserreich: Die Attraktivität Preussen-Deutschlands für konservative und liberale Kreise in Grossbritannien, 1860–1914* (Wiesbaden, 1974), 29, 259. Although J. A. Schumpeter can hardly be described as an inspiration, for his famous essay on imperialism first appeared in 1918–19, there exists a striking parallel between the views expressed by Bernstein and those of the young Schumpeter; Schumpeter, *Imperialism and Social Classes*, trans. H. Norden (New York, 1951). Nowhere is Bernstein's anticipation of Schumpeter more pronounced than in a lecture Bernstein delivered before the Vienna Sociological Society in November 1912; see “Der Imperialismus, seine Bedeutung und sein Zukunft,” *Arbeiter-Zeitung* (Vienna), November 15, 1912. Thus it may well be, as W. L. Langer observed in 1935, that the credit for Schumpeter's insights ultimately belongs to Hobson; see Harvey Mitchell, “Hobson Revisited,” *Journal of the History of Ideas*, 26 (1965): 405. Worth a study in itself is the extent to which the views of Hobson, Bernstein, and Schumpeter may or may not have had a common ancestry in Herbert Spencer's distinction between militant and industrial societies; see David Wiltshire, *The Social and Political Thought of Herbert Spencer* (Oxford, 1978), 243–55.

⁴³ In 1894, for instance, he still maintained, as befitted an orthodox Marxist in the shadow of a still-living Engels, that the cosmopolitanism of bourgeois radicalism was “a very uncertain thing, always in danger of turning into its opposite from one day to the next,” its internationalism extending no further than its business interests; Bernstein, “Am Gedenktag der Internationale,” *NZ*, 12 (1893–94), 2: 807, 809.

policy or for democratic reform within the Reich.⁴⁴ Particularly in the area of foreign relations, German liberals of all persuasions were almost diametrically opposed to the kind of liberal internationalism that Bernstein associated with free trade.⁴⁵ As Brentano's biographer has observed, "men like Brentano, Barth, Naumann, and Weber . . . not only acquiesced in, but actively supported the tragic course of Germany's foreign policy." Largely for this reason Bernstein's overwhelmingly positive appreciation of British liberalism contrasted markedly with his fundamentally negative attitude—apparent in the chief exegete of revisionism no less than in the erstwhile orthodox Marxist—toward German liberalism, which he despised for its timidity and class egoism both in theory and in practice.⁴⁶

Believing liberalism in Germany to be more reactionary than it was elsewhere, and to be growing steadily more so, he drew the practical conclusion that it was incumbent on Social Democracy to display a conciliatory spirit and to take the initiative in forging links between the labor movement and middle-class parties. Here he pinned his hopes principally on the new middle class or white-collar proletariat.⁴⁷ The necessity of a bourgeois alliance he derived from two main sources. One was his conviction that revolution was no longer either possible or desirable, which left the parliamentary road as the sole alternative way forward, and this demanded the formation of a bloc of the left as "the indispensable precondition . . . for a sweeping democratization of political life."⁴⁸ Second, he periodically succumbed to bouts of extreme and unwarranted optimism regarding the possibilities of a socialist-liberal alliance. On such occasions he either generalized from his enthusiasm for English liberal experience or mistook individual exceptions (like Brentano) for an earnest of what might yet, somehow, become reality, even in Prussia-Germany.⁴⁹

Just such an exception was Friedrich Albert Lange (1828–75), a Kantian democrat who is often alleged to have had a major impact on Bernstein's

⁴⁴ Bernstein, "Eine demokratische Bibliothek," *Frankfurter Zeitung*, January 18, 1913, and "The Meaning of the Bülow Crisis," *The Nation*, July 3, 1909, p. 191. On the *Hansabund*, see Siegfried Mielke, *Der Hansa-Bund für Gewerbe, Handel, und Industrie, 1909–14* (Göttingen, 1976).

⁴⁵ Bernstein regarded the nationalism of the liberals as the main obstacle to a bloc of the left and to parliamentary government in the Reich: Bernstein, "Political Scene-Shifting in Germany," *The Nation*, July 23, 1910, p. 595.

⁴⁶ James J. Sheehan, *The Career of Leo Brentano* (Chicago, 1966), 180–81; and Bernstein, "The German Elections and the Social Democrats," *Contemporary Review*, 91 (1907): 481, 486, and *Verhandlungen DR*, May 15, 1914, p. 8887. Also see James J. Sheehan, *German Liberalism in the Nineteenth Century* (Chicago, 1978), 272–83. On the antidemocratic inclinations of liberals like Brentano, see Walter Struve, *Elites against Democracy: Leadership Ideals in Bourgeois Political Thought in Germany, 1890–1933* (Princeton, 1973), 53–219.

⁴⁷ Bernstein, "Wird die Sozialdemokratische Volkspartei?" *SM* (1905), 2: 671. "The Pull towards the Left in Germany," *The Nation*, November 6, 1909, p. 241. "The Eve of the Prussian Revolution," *ibid.*, April 2, 1910, pp. 15–16. "The Death of a Deformed Reform Bill," *ibid.*, June 4, 1910, pp. 347–48, and *Was ist Sozialismus?* (Berlin, 1918), 19. This economically dependent class was, he noted, growing in size and in political radicalism. He hoped that it could be won over to Social Democracy or used as a lever for radicalizing or splitting the National Liberals and even the Center, facilitating the creation of a grand bloc embracing Social Democrats, National Liberals, left Liberals, and perhaps elements of the Center party as well.

⁴⁸ Bernstein, "Politische Schwarzmalerei," 541.

⁴⁹ For two blatant examples of such generalization, in 1890 and 1898, see Bernstein, "Carlyle und die sozialpolitische Entwicklung Englands," *NZ*, 9 (1890–91), 1: 666, 733–35, and "Kritisches Zwischenspiel," *NZ*, 16 (1897–98), 1: 750.

revisionism.⁵⁰ Certainly there were striking parallels and major points of contiguity to which the budding revisionist could hardly have been indifferent. Lange's empiricism, his hostility to speculative philosophy in general and to Hegelianism in particular, his eclecticism, and especially his blending of Darwin, Malthus, and Marx; his individualism, agnosticism, cosmopolitan patriotism, and interest in the agrarian question; his insistence on working-class self-help and antistatism; his moral idealism and humanism; his hostility to German liberalism for its tepid defense of political principle and its neglect of the social question; and, above all, his concept of socialism as identical to the cooperative idea, thus subsuming democracy, perceived both as a means and as an end and, therefore, also as gradualism and free associations—all this cannot have failed to strike a sympathetic chord with Bernstein.

Yet Bernstein was never a neo-Kantian, as both he and his friends have noted. Karl Vorländer, who was intimately acquainted both with neo-Kantianism and with Bernstein, reached the conclusion that Bernstein was "far removed from Kant's method and Kantian ethics" and that his "return to Kant" rested, "in the last resort, on a misunderstanding."⁵¹ Bernstein's discovery of Lange, in the forefront of the neo-Kantian revival, seems to have dated not from the Karl Höchberg period (the late 1870s) but from January 1892, when Bernstein informed Kautsky that he had been reading works on and by Lange in the British Museum. The result was a series of articles published in the *Neue Zeit*, essentially an extended review of a recent popular biography by O. A. Ellissen. What these articles indicate is that "Bernstein was still very reserved toward neo-Kantianism."⁵²

Writing six years later as a full-blown revisionist, he used the slogan "Back to Kant" but granted it only limited validity in its application to socialist theory. In his *Voraussetzungen des Sozialismus und die Aufgaben der Sozialdemokratie* he cited Lange in his attack on the Hegelian dialectic and translated "Back to Kant" as "Back to Lange," but what he really meant by this was a mere call for emulation of "the distinguishing union in Lange of an upright and intrepid championship of the struggle of the working classes for emancipation with a large scientific freedom from prejudice."⁵³ In short, the only conclusion warranted by the available evidence is that Lange's influence may have contributed somewhat to Bernstein's abandon-

⁵⁰ On Lange, see Hermann Lübke, *Politische Philosophie in Deutschland* (Basel, 1963), 92–99; and Thomas E. Willey, *Back to Kant: The Revival of Kantianism in German Social and Historical Thought, 1860–1914* (Detroit, 1978), 83–101. Scholars who have suggested that Lange significantly influenced Bernstein include Hans-Josef Steinberg, Thomas Meyer, Peter Gay, and Sven Papcke; see Steinberg, *Sozialismus und deutsche Sozialdemokratie: Zur Ideologie der Partei vor dem Ersten Weltkrieg* (3d rev. edn., Bonn, 1972), 90–91, 98; Meyer, *Bernsteins konstruktiver Sozialismus*, 111–22; Gay, *The Dilemma of Democratic Socialism*, 154; and Papcke, *Der Revisionismusstreit und die politische Theorie der Reform* (Stuttgart, 1979), 76, 114.

⁵¹ Vorländer, *Kant und Marx* (Leipzig, 1911), 189; and Bernstein, *Entwicklungsgang eines Sozialisten* (Leipzig, 1924), 40. Also see Willey, *Back to Kant*, 176; and Helga Grebing, *Der Revisionismus: Von Bernstein bis zum "Prager Frühling"* (Munich, 1977), 43. Yet Bernstein continues to be described as a neo-Kantian. See J. W. Burrow, *Evolution and Society* (Cambridge, 1966), 262; and R. Steigerwald, *Bürgerliche Philosophie und Revisionismus im imperialistischen Deutschland* (Frankfurt, 1980), 30, 33.

⁵² Steinberg, *Sozialismus und deutsche Sozialdemokratie*, 90; and Bernstein, "Zur Würdigung Friedrich Albert Langes," *NZ*, 10 (1891–92), 2: 68–78, 101–09, 132–41. Also see Vorländer, *Kant und Marx*, 180. The biography in question is that by Otto Adolf Ellissen, which Bebel also deemed "a commendable book"; Ellissen, *Friedrich Albert Lange: Eine Lebensbeschreibung* (Leipzig, 1891); and Bebel, *Aus meinem Leben* (5th edn., East Berlin, 1978), 154. Lange's main works are *Die Arbeiterfrage* (1865) and *Die Geschichte des Materialismus* (1866).

⁵³ Bernstein, "Das realistische und das ideologische Moment im Sozialismus" (*NZ*, 1898), in *Zur Theorie und Geschichte des Sozialismus*, 2: 124, and *Die Voraussetzungen des Sozialismus*, 54, 257.

ment of orthodox Marxism, but that influence only reinforced other and more significant forces impelling him toward revisionism. Indeed, there is reason to doubt whether this intelligent, earnest, and industrious autodidact ever really acquired a solid grasp of Lange, to say nothing of Kant. In 1905, for example, he still referred to Lange as a “socialist of the chair,” and in 1909 he believed that Lange “gave materialism a prominent place in science,” when in fact his putative mentor had set out to demolish the cardinal points of the materialist argument and to demonstrate that this argument was inherently and fallaciously monistic.⁵⁴

This is by no means to deny Lange all influence on Bernstein. Like Bebel and Kautsky, Bernstein exhibited great personal respect for the man and his views, referring to him as the “noble F. A. Lange.” What can reasonably be claimed is that through Lange he “discovered an affinity between revisionism and Marburg neo-Kantianism which offered the possibility of a timely convergence of bourgeois reformism with the working-class movement.” Exemplars like Lange gave him hope, as did T. H. Green, the later J. S. Mill, and David Lloyd George, at a time when he could no longer accept the validity of the collapse theory, that class war and revolution were not the only roads to radical social transformation. No doubt Bernstein found Lange all the more acceptable by virtue of Lange’s affinity with English liberalism; for Lange, too, was an admirer of British labor organizations and of English social thought, especially that of John Stuart Mill.⁵⁵

That Bernstein could appreciate Lange’s ethical approach to the social question without embracing neo-Kantianism must also be explained by reference to his English experience. The people with whom he had most contact and whom he most admired during his London exile were not the ineffectual SDF sectarians or the Fabians (as the “best informed” of English socialists, the Fabians evidently offered congenial, stimulating, and instructive company) but ethical socialists like Keir Hardie and Ramsay MacDonald, who drew their inspiration not from Kant but from the New Testament. Bernstein defended them stoutly. In 1890 he noted that British Christian Socialists were of an altogether different stamp from Stöcker, Distelkamp, and Treitschke. Many of them had participated actively in the workers’ class struggle against capital and distinguished themselves in the organization of the new unions. In 1897 he sang their praises more loudly still: “if a large section of English democracy draws its ethics from the New Testament rather than from some atheistic treatise, these ‘bigots’ and ‘pharisees,’ or whatever one wishes to call them, have performed infinitely greater services for liberty in Europe than we enlightened Germans have so far done.” In 1904 he maintained that Keir Hardie’s socialism bore “a much more pronounced *ethical* hue than that of German Social Democracy,” to which he added, “Emphasis on the ethical factor may be a sign either of a backward movement or of the more advanced conditions with which it has to deal. Here both factors coalesce.”⁵⁶ Clearly Bernstein was more impressed by

⁵⁴ Bernstein, *Die heutige Sozialdemokratie in Theorie und Praxis* (Munich, n.d.), 8, as quoted in Meyer, *Bernsteins konstruktiver Sozialismus*, 111 n. 14, and “The Revival of Will in German Literature,” *The Nation*, January 9, 1909, 576.

⁵⁵ Bebel, *Aus meinem Leben*, 85–86; and Steinberg, *Sozialismus und deutsche Sozialdemokratie*, 52.

⁵⁶ Bernstein, “Carlyle und die sozialpolitische Entwicklung Englands,” 730–32, “Kreta und die russische Gefahr,” 15, and “Nationale Besonderheiten und internationale Sprache,” *SM* (1904), 2: 893–94.

Lange's social activism than by his Kantianism, more by British than by German ethical socialism.

As to Bernstein's liberal internationalism, there is no evidence of more than a certain compatibility of outlook with Lange. No more than Bernstein was Lange a doctrinaire free-trader or devotee of Manchesterism of the laissez-faire sort. Like Bernstein, Lange was an enemy of chauvinism and an adherent of the typically naive, nineteenth-century view that there was no inherent contradiction between a progressive patriotism and a healthy cosmopolitanism.⁵⁷ Although Bernstein must have found such views congenial, they were not echoed or otherwise registered in his discussion of foreign policy and tariff problems.

WITHIN THE SPD, BERNSTEIN'S FREE-TRADE INTERNATIONALISM was closest to the position of the party center, which, through Point Ten of the Erfurt Program, and still more by the onerous burden that the Bülow tariff inflicted on proletarian living standards, was firmly committed to free trade. In matters of taxation and tariff policy, he stood, as he later maintained in his autobiography, squarely on the ground of the party orthodoxy.⁵⁸ Not infrequently, center-orthodox spokesmen also drew the same kinds of internationalist inferences that Bernstein did. August Bebel, for example, told the party congress in 1911, "German industry and German commerce have expanded enormously. Great amounts of French, English, and American capital have been invested in Germany, whereas German capital goes abroad to be invested there. I openly admit that perhaps the greatest guarantee of world peace lies in this international export of capital." And at the subsequent party congress Hugo Haase also welcomed the trend toward economic interdependence as "a factor working against the warmongers."⁵⁹

Bernstein recognized, however, that, "until very recent times, representatives of socialism [had been] regularly hostile to free trade, declaring themselves sometimes conditionally and sometimes unconditionally for protective tariffs." In 1896 he had deemed it "curious the way socialist heads can still be captivated by the protectionist idea." Later, when it became apparent that such heads included a substantial body of his revisionist and reformist colleagues, he excoriated them for their "intellectual reaction," their "retreat from theoretical to crudely empirical thinking."⁶⁰ Apart from Eduard David and Albert Südekum, almost none of the prominent revisionists shared Bernstein's free-trade outlook. Indeed, the overwhelming majority of revisionists were, like Joseph Bloch, both protectionists and Anglophobes. On the left, of course, Bernstein had no friends at all, and least of all on the tariff question.

⁵⁷ Willey, *Back to Kant*, 83, 85, 91, 92, 94, 95; and Toni Offermann, *Arbeiterbewegung und liberales Bürgertum in Deutschland, 1850–1863* (Bonn, 1979), 217–18. On Bernstein's treatment of the national question, see Hans Mommsen, *Arbeiterbewegung und nationale Frage* (Göttingen, 1979), 109–24.

⁵⁸ Bernstein, *Entwicklungsgang eines Sozialisten*, 43.

⁵⁹ Bebel, *Jena Protokoll* (1911), 345; and Haase, *Chemnitz Protokoll* (1912), 412.

⁶⁰ Bernstein, *DdS*, I (1902): 101; "Deutschland als Konkurrent Englands," 757, and "Zollfreier internationaler Verkehr," 832.

To radical Marxists like Rosa Luxemburg the issue was, quite simply, neither protectionism nor free trade but socialism. Whereas some accommodation between Bernstein and Hobson was, at least theoretically, within the bounds of possibility, for Hobson considered imperialism “a depraved *choice* of national life” and eventually joined the Angellites in their crusade for peace as a common-sense dictate of rationality,⁶¹ a *modus vivendi* between Bernstein and the radical Marxists was unthinkable. To Hilferding and Luxemburg imperialism was not a matter of choice but of economic necessity, and Bernstein’s Cobdenism appeared to them as naive, illusory, and even reactionary.

Since he was not a socialist revolutionary like Luxemburg, Bernstein could argue, as he did before the Reichstag in May 1912, that international relations were the supreme and central issue of the age.⁶² That he did not, before 1914, subject such issues as the nationality, militarism, imperialism, and tariff questions to the kind of searching and comprehensive analysis provided by Otto Bauer, Rudolf Hilferding, Karl Liebknecht, or Rosa Luxemburg is indicative less of a lack of interest in this problem complex than it is a reflection of his abandonment of the Marxist method for a typically reformist preoccupation with the minutiae of *Tagespolitik*. If his approach now appeared relatively superficial, eclectic, pragmatic, and empirical, this is because he had become, through the influence of his English environment, and yet largely unknown even to himself, a liberal democrat. This transformation is abundantly clear in his treatment of tariff policy, less unambiguously so in relation to other problems such as those of nationality and militarism, where he still made some effort to remain faithful to Engels, if not to Marx. Bernstein’s treatment of all these issues is permeated with a strain of utopian optimism and idealism, an attachment to democracy as a panacea, and a firm commitment to the Cobdenite principles of free-trade internationalism.

In all circumstances his indebtedness to Marx was more apparent than real. At best, he was guilty of the same charge of idolatry that he so frequently brought against the party center, for he, as much as anyone, used Marx as little more than a grab-bag of convenient quotations. In fact, he was true to neither the method nor the spirit of Marx. This circumstance goes a long way toward explaining why Bernstein’s position on many issues, and especially on those of foreign policy, turned out to be remarkably close to the position of the center-orthodoxy, as represented by Bebel and Kautsky. Similarly, Bernstein’s essential liberalism was the root cause of his poor relations with the bulk of his fellow revisionists—reformists like David and Südekum, no less than illiberal social imperialists like Joseph Bloch and Karl Leuthner—most of whom went much farther and faster than he along the road to protectionism, nationalism, and militarism. Even in the most capable hands, and Bernstein was always willing to admit that his were less

⁶¹ Hobson, *Imperialism*, 368 (italics added); and Taylor, *The Trouble Makers*, 101.

⁶² Bernstein, *Verhandlungen DR*, May 14, 1912, p. 1996. On his treatment of the problem of imperialism, see H.-C. Schröder, “Eduard Bernsteins Stellung zum Imperialismus vor dem Ersten Weltkrieg,” in Heimann and Meyer, *Bernstein und der demokratische Sozialismus*, 166–212; and R. A. Fletcher, “A Revisionist Looks at Imperialism: Eduard Bernstein’s Critique of Imperialism and *Kolonialpolitik*, 1900–1914,” *Central European History*, 12 (1979): 237–71.

capable than others,⁶³ liberalism has never distinguished itself as a rigorously systematic *Weltanschauung*. It should therefore surprise no one that Bernstein's thought, caught in the vice of his *soi-disant* Marxism and his new-old liberal commitment to "the facts," should be shot through with errors, inconsistencies, and illogicalities.

With the wisdom of hindsight we may observe that in his case one of the blunders that probably did a great deal of damage was his conviction that it was possible to be both a German patriot and an internationalist, a reasonable nationalist and a good European. As British free-trade imperialists had done in early and mid-Victorian England, and as many British workers and imperialists continued to do in the late Victorian era, Bernstein mistakenly took it for granted that in respect of German imperialism the world was large enough to accommodate a spiritually and economically conceived greater fatherland without involving Germany in undesirable collisions and entanglements with other European powers.

IT SEEMS FAIR TO SAY that Bernstein had scant affection for German liberalism and little understanding of neo-Kantianism. Insofar as the apostate Marxist *cum* revisionist sought a mentor or educator, he searched not among the tribal gods of Germany but in his immediate environment, which at the time happened to be British. Thus, his intellectual pantheon came to include such *numina* as Jeremy Bentham, Richard Cobden, and John Stuart Mill. All this is highly revealing of the character of Bernstein's revisionism and of his thought generally. It also sheds considerable light on two further transitions in Bernstein's political career: the transformation of the revisionist "half-and-half supporter of imperialism," as Georg Ledebour dubbed him at Mainz in 1900, into the social pacifist of the war years, incongruously rubbing shoulders with Karl Liebknecht and Rosa Luxemburg as well as Kautsky and Hugo Haase, and what Bernstein himself termed his effective "political death" in the postwar years.⁶⁴

⁶³ According to Bebel, this readiness to admit his own mistakes was one of the main reasons why Bernstein was always "moulting," or changing his views; Bebel to Bernstein, October 22, 1898, International Institute for Social History, Amsterdam, Bebel Papers, A6. For Bernstein's admission that his was more an "analytical" than a "synthesizing" brain, see *Entwicklungsgang eines Sozialisten*, 7.

⁶⁴ Ledebour, *Mainz Protokoll* (1900), 167; and Bernstein to Kautsky, November 9, 1927, as quoted in Gay, *The Dilemma of Democratic Socialism*, 296.

Iranian Revolutions in Comparative Perspective

NIKKI R. KEDDIE

THE IRANIAN REVOLUTION of 1978–79 shocked the world and set in motion a search for causes. Most of the resulting analyses tend to locate the origins of the revolution in the errors of the shah and of various Americans, although some scholarly works assay socioeconomic explanations for the upheaval. Enough time has now passed to permit a greater range of investigations, and one written from the comparative perspective ought to be revealing. This essay will venture two types of comparison: (1) internal—comparison, on a few significant points, with other Iranian rebellions and revolutionary movements since 1890—and (2) external—comparison, more briefly, with other great world revolutions, employing theories of revolution that seem to fit the Iranian case. Both are difficult and tentative exercises, because Iran’s “Islamic revolution” appears to bear little ideological resemblance to revolutions in the West or to Iran’s “constitutional revolution” of 1905–11.

Western revolutions have tended, especially in their radical phase, to shift to the left and move toward secularism; even if religious ideology was dominant, as in the English Civil War of the 1640s, it was not fundamentalist, nor was it proclaimed by the leaders of the existing religious structure. In the Islamic revolution, however, much of the leadership came from the orthodox clergy, who propounded a return to Islamic fundamentalism.¹ Even recent socioeconomic theories of revolution, which tend to stress the role of the peasantry (probably because of the partial example of Russia and the clearer ones of China and other Third World countries), do not apply to either Iranian revolution; a peasantry living mainly on arid or semi-arid land, dependent on landlords or others for irrigation, and unprotected by forest or mountain shelter for guerilla warfare seems to produce neither the middle peasantry nor the close associations needed to organize politically. By contrast, the supposed impermeability of modern cities to successful mass revolution, especially in the absence of peasant support, was disproved in both Iranian revolutions; the

Earlier versions of this paper were delivered in 1981 at the University of California, Irvine, Seminar on Social History and Theory and at the Conference on Islam and Social Movements, University of California, Berkeley. Thanks are due to Ira Lapidus, Edmund Burke, and Gene R. Garthwaite for suggesting revisions to the manuscript and to Eric Hooglund, Andrew Newman, and the editors and staff of the *American Historical Review* for their help in preparing it for publication.

¹ Fundamentalism is here used for movements calling for a return to scriptural religion. Fundamentalist movements are mostly nineteenth- and twentieth-century phenomena, and none of them aims at or achieves a true re-creation of the religious past.

cities were vulnerable in 1905–11 largely because the shah had no significant military force and in 1978–79 owing not only to the shah's miscalculations but also to the amazing unanimity and organization of the urban population against him.

Iranians, who in peaceable periods seem eager to please and loath to disagree openly with the powerful, whether in the home or with political superiors, have in the last ninety years engaged in an unusual number of large-scale popular revolts and revolutions. With the exception of several northern provincial revolts after World War I and the large demonstrations against the shah in the early 1960s, all of these rebellions spread to Iran's major cities, and some encompassed tribal areas as well. Indeed, Iran stands in the forefront of rebellious and revolutionary countries in the twentieth century—unmatched, to my knowledge, in the Muslim, Hindu, or Western world for the number and depth of its movements; only China, Vietnam, and possibly Russia provide competition.

This claim may be unexpected to some, not only because modern Iranian history is not generally known but also because Iran's two major twentieth-century revolutions, and especially the second, appear so aberrant. They do not fit very closely widespread ideas of what modern revolutions should be like. Yet there is no doubt that the Islamic revolution in 1978–79 provided a thoroughgoing overthrow of the old political, social, and ideological order, although what will replace it is not yet clear. And the constitutional upheaval of 1905–11 was massive enough in participation and important enough in altering the political system to deserve the name "revolution." Several movements that were not revolutions contained revolutionary elements. The mass rebellion against a British tobacco concession in 1890–92, the reformist-autonomist revolts in the provinces of Gilan, Azerbaijan, and Khorasan after World War I, the rebellions in Azerbaijan and Kurdistan after World War II, the mass-supported oil nationalization movement under Mosaddeq that ruled the country from 1951 to 1953, and the popular antigovernment demonstrations of the early 1960s all involved, to a greater or lesser extent, efforts to throw off foreign control over the Iranian economy and to build an independent society and state.

TO ATTEMPT ANYTHING MORE THAN SUPERFICIAL COMPARISONS among Iran's rebellions, not to mention comparisons between some of these upheavals and revolts in other Muslim and non-Muslim countries, requires locating the various Iranian movements of the last century within the framework of modern Iranian history. Under the Qajar dynasty (1796–1925), Iran was increasingly subject to Western economic penetration and domination, particularly by Great Britain and Russia. As in many Third World countries, Western powers exacted from Iran treaties that limited customs duties to 5 percent, thus virtually creating a free trade area for Western imports, which often undersold Iranian handicrafts. Although Oriental carpets began to be a significant export around 1875, it is unlikely that the rise in carpet exports compensated for the fall in production of other crafts and the consequent discontent and displacement of their artisans.

In the same period the decrease in Iran's handicraft exports was partially offset

by rising raw material and agricultural exports, particularly opium, cotton, and fruits and nuts. The commercialization of agriculture and carpets, which continued in the Pahlavi period (1925–79), increased economic stratification between the owners of land, water, or workshops and those who worked for them. Whether there was general immiseration or an increase in prosperity is a question on which scholars of the Qajar period have disagreed.² But the increase in stratification and the peasants' increased vulnerability to famine, owing to their dependence on land planted in cash crops like opium that were subject to bad market years, brought new sources of discontent to the peasantry, just as the displacement of craftsmen contributed to the grievances of middle-level urban residents. Iran did, however, have an advantage over countries like Egypt and Turkey, which had a much larger trade with Europe and far more European residents, in that the native Iranian bazaar structure remained largely intact. And wealthy import-export and local merchants and moneylenders proved important in every Iranian revolution.

The Qajars did much less than the Middle Eastern rulers of, for example, Turkey, Egypt, and Tunisia in trying to strengthen the central government and the army in order to resist further encroachments either by Western powers or by their own neighbors. Turkey saw a long series of efforts, beginning in the eighteenth century, to strengthen both its military and its technical and educational support structure; the first stage of those efforts culminated in the reforms of Sultan Mahmud in the 1820s and 1830s. And Egypt under Muhammad Ali saw even more significant transformations until Western powers limited both the economic independence and military strength of the Egyptian government in the 1840s. Iran had no parallel developments. Largely abortive reforms under Crown Prince Abbas Mirza (d. 1833) and chief ministers Amir Kabir (d. 1851) and Mirza Hosain Khan (d. 1881) left Iran without a modernized army, bureaucracy, and educational

² Gad G. Gilbar has noted that in the late nineteenth and early twentieth centuries grain production fell greatly, so that wheat and barley changed from export to import commodities. The consequent rise in the price of foodstuffs resulted in bread riots, often led by clerics, and demands that grain exports be discontinued. Large-scale merchants encouraged transferring production from grain and cotton to opium, which according to Gilbar improved the welfare of almost all groups engaged in opium production and marketing. Gilbar, "Persian Agriculture in the Late Qajar Period, 1860–1906: Some Economic and Social Aspects," *Asian and African Studies*, 12 (1978): 321–65. Roger T. Olson's more detailed study of opium growing and sale indicates that opium production aided the better off but subjected poorer peasants and townspeople to large price fluctuations in agricultural staples and, occasionally, to famine. Emphasis on opium clearly increased social and economic stratification. Olson, "Persian Gulf Trade and the Agricultural Economy of Southern Iran in the Nineteenth Century," in Michael E. Bonine and Nikki R. Keddie, eds., *Modern Iran: The Dialectics of Continuity and Change* (Albany, N.Y., 1981), 173–89. Claiming "a certain improvement in the standard of living of the peasants," Gilbar stated, "First, there are various pieces of evidence to show that peasants in many areas had a more diversified daily diet, consuming commodities which they could hardly have afforded before. Sugar, tea, tobacco, and opium are perhaps the best examples of articles which peasants consumed in large quantities in the late 19th century." There are problems with Gilbar's analysis. Not only has he presented evidence for four commodities that are unhealthful and should not be cited as proof of a "more diversified daily diet," but he has also failed to note that these four items quite likely replaced nutritious ones that, by his own evidence, were becoming expensive. The experience of most modern countries suggests that a kind of dietary Gresham's Law works to supplant fruits, vegetables, and meat when less nutritious and less costly products become available, and British documents as well as travel literature support the contention that the experience of modern Iran proves no exception. Some of Gilbar's other points are more convincing, but the argument is far from settled, and Willem M. Floor, in a paper at a 1981 conference at Harvard on the Iranian revolution of 1905–11, argued for the general immiseration of Iranians in the prerevolutionary period. For my arguments and conclusions, see Nikki R. Keddie, *Roots of Revolution: An Interpretive History of Modern Iran* (New Haven, 1981), esp. 54–57.

system. The small Russian-officered Cossack Brigade, founded in 1879, remained the Qajars' only modern military force.

This lack of change is not altogether surprising. Iran had much less contact with the West than did Middle Eastern countries bordering on the Mediterranean and had a very arid terrain with a scattered population. As a result, the country was very difficult to subject to centralized control. Other countries in similar circumstances—Afghanistan and Morocco, for example—also saw relatively little centralization or modernization in the nineteenth century. The shahs had to permit considerable devolution of power to groups not totally tied to the center. Among these were nomadic tribes (often organized into confederations for the main purpose of dealing with the authorities), whose mobility, mastery of gunfighting on horseback, separate languages and cultures, and geographical location (frequently near the borders) made them semi-autonomous units. Their ties to the government were frequently limited to annual payments or to cavalry duties in case of war. Even some local governors or mayors had considerable authority, although the central government exercised increasing control over them, especially under Naser ed-Din Shah (1848–96).³

The lack of centralization in Iran was also dependent on the increasing power and pretensions of the Shi'i ulama. By the early nineteenth century, after a long prior evolution, the *usuli* or *mujtahidi* school of ulama won out over the rival *akhhbari* school. The latter claimed that individual believers could themselves understand the Quran and the Traditions (*akhbar*) of the Prophet and the Imams and did not need to follow the guidance of mujtahids, who claimed the right of *ijtihad* ("effort to ascertain correct doctrine"). The usulis, in contrast, claimed that, although the bases of belief were laid down in the Quran and the Traditions, learned mujtahids were still needed to interpret doctrine for the faithful. As usuli doctrine developed, particularly under Mortaza Ansari, the chief *marja'-e taqlid* ("source of imitation") of the mid-nineteenth century, every believer was required to follow the rulings of a living mujtahid, and, whenever there was a single chief mujtahid, his rulings took precedence over all others.⁴ The usuli ulama have a stronger doctrinal position than do the Sunni ulama. While not infallible, mujtahids are qualified to interpret the will of the infallible twelfth, Hidden Imam.

In addition to doctrinal power, which extended to politics as well as religion and law, the Iranian Shi'i ulama had economic and social power that similarly exceeded that of the ulama in most Sunni countries. Shi'i ulama, unlike most Sunni ulama, directly collected and dispersed the *zakat* and *khums* taxes, and they also had huge *vaqf* mortmains as well as personal properties, controlled most of the dispensing of justice, were the primary educators, oversaw social welfare, and were frequently courted and even paid by rulers. Although most of the ulama were often on good

³ See Gene R. Garthwaite, *Khans and Shahs: The Bakhtiari in Iran* (Cambridge, 1983), and "Khans and Kings: The Dialectics of Power in Bakhtiari History," in Bonine and Keddie, *Modern Iran*, 159–72; Willem M. Floor, "The Political Role of the Luti in Iran," *ibid.*, 83–95; Ervand Abrahamian, *Iran between Two Revolutions* (Princeton, 1982); John Malcolm, *The History of Persia*, 2 (London, 1815); and Keddie, *Roots of Revolution*, chaps. 2–3. For additional information, see the numerous anthropological articles on Iranian nomads.

⁴ Especially see Juan R. Cole, "Imami Jurisprudence and the Role of the Ulama: Mortaza Ansari on Emulating the Supreme Exemplar," in Nikki R. Keddie, ed., *Religion and Politics in Iran* (New Haven, 1983), 33–46; and Mortaza Ansari, *Sirat an-Najat* (n.p. [Iran], A.H. 1300 [1883]).

terms with the crown, they resisted Qajar encroachments on their power, whereas in most Sunni states the ulama became more and more subordinate to the government. Some of the Iranian ulama worked for the state, but as the nineteenth century progressed conflicts between important ulama and the secular authorities increased.

The relative independence of the ulama facilitated their alliance with the bazaar—a term used to designate those engaged in largely traditional, urban, small-scale production, banking, and trade—and its artisans, merchants, and moneylenders. The bazaar has long been the economic, social, and religious center of towns and cities, and even in recent times has encompassed a large population and share of the economy. As early as the 1830s bazaaris complained to the government about the large-scale importation of foreign manufactures, which undermined their own production and trade. Given the long-term trade treaties limiting Iranian tariffs, there was little the government could do, short of risking war with the Western powers, even if Iranian rulers had been more energetic.

Regardless, then, of whether certain individuals or groups were better or worse off as the result of the Western impact on Iran, including British and Russian protection of Qajar rule, various groups in society had reason to be actively discontented with the Qajars and with Western incursions. Those craftsmen who were displaced had clear grievances, and many of them petitioned the government for redress. Even those merchants who prospered, however, saw that Western merchants received favored treatment—Westerners were exempt, for example, from road and municipal taxes that Iranian merchants had to pay. And the ulama were opposed to the limited steps the Qajars took toward Western education—missionaries were allowed, for instance, to teach Christians in Iran. The ulama also objected to steps toward reform and to concessions granted to Westerners. Except in the densely populated areas of heavy rainfall, like Gilan and Mazenderan on the Caspian, however, peasants were generally too scattered and too subject to landlord control of land and water to organize or coordinate movements of discontent, although those who migrated and became urban subproletarians were willing participants in urban-based rebellions, not only in the Qajar period but also, and to a greater extent, in the revolution of 1978–79.⁵

⁵ Especially see Eric Hooglund, "Rural Participation in the Revolution," *Middle East Research and Information Project Reports*, 87 (1980): 3–6; and Mary Hooglund, "One Village in the Revolution," *ibid.*, 7–12. Ervand Abrahamian and Farhad Kazemi in a seminal article have stressed what I see as partly derivative features of the Iranian peasantry in explaining its generally nonrevolutionary character—namely, the absence of significant outside market ties and of a substantial middle peasantry, which has elsewhere been found crucial in leading peasant revolts; Abrahamian and Kazemi, "The Non-Revolutionary Peasantry in Modern Iran," *Iranian Studies*, 11 (1978): 259–304. The authors have understated, though less than many social and comparative historians, the roles of geography, technology, and ecology. The arid climate of most of Iran has meant that its peasants have always been less densely settled and, hence, more difficult to organize; they have been, as the authors have noted, more than usually dependent on relatively expensive underground irrigation systems that easily came under landlord control. Tribal khans' dominance over peasants may also have decreased peasant revolutionary potential. Local peasant rebellions have been frequent but could not spread, given scattered villages and strong local power. The densely populated, productive, and organized lands of China, for example, were conducive to a far more organized and organizable, and frequently rebellious, peasantry. A strong middle peasantry has largely been due to such an environment. This argument based on environment is supported by Iran's own experience; Iran's most revolutionary peasants are found in the high-rainfall, densely settled, rice-growing province of Gilan, as the authors discussed, though with less stress on ecology. Abrahamian and Kazemi have covered all of the important questions, but I would emphasize more the ecological base of the issue.

Among the discontented in the nineteenth century was also a small but growing group of intellectuals, many of whom had mercantile or government positions, who learned of Western ways. Frequently their knowledge of the West was obtained second hand, by travel to India, Istanbul, or Egypt or by temporary migration to Russian Transcaucasia. Hundreds of thousands of Iranians, mostly workers, settled semi-permanently in the Transcaucasus, which also supported a few Iranian intellectuals. Several educated Iranians, most notably Mirza Malkum Khan and Sayyed Jamal ed-Din "al-Afghani," also traveled as far as France and England. Those who went abroad were generally struck by Western economic development, comparative justice, and lack of arbitrary rule; their manuscript writings contain praise of Western ways and criticism of Iran's autocratic rulers, petty officials, venal clerics, and arbitrary courts, and of the low status of women.⁶

To a large degree, the recurring alliance between the bazaaris and many of the ulama on the one hand and secularized liberals and radicals on the other has been based on the existence of common enemies—the dynasty and its foreign supporters—rather than on any real agreement about goals. The ulama wanted to extend their own power and to have Shi'i Islam more strictly enforced; the liberals and radicals looked for greater political and social democracy and economic development; and the bazaaris wanted to restrict favored foreign economic status and competition. The alliance formed by many of the ulama, the bazaaris, and a few secular intellectuals first showed its power following the issuance of a tobacco concession in 1890; Iran granted to a British subject a full monopoly on the purchase, sale, and export of all tobacco grown in Iran. Not only did this follow a whole series of concessions to Europeans, but it also covered a widely grown, exported, and profitable crop rather than previously unexploited products, like most minerals. Thus, growers and merchants became aroused by the threat to their livelihood as well as by nationalistic fervor. Active and often massive protests in most of Iran's cities in 1891, largely led by ulama in partnership with bazaaris (and with some Russian behind-the-scenes encouragement), culminated in a successful boycott of tobacco dealing and smoking (as against the will of the Hidden Imam). The shah was forced to cancel the tobacco monopoly in early 1892.⁷

The tobacco rebellion of 1890–92 shared with later revolutionary and rebellious movements a substantial anti-imperialist and antiforeign component. Although this component is also found in most of the world's colonies and dependencies, anti-imperialism seems to have been stronger and to have resulted in more mass rebellions and revolutions in Iran than in other Middle Eastern countries, with the possible exception of Afghanistan. Despite the lesser degree of direct control that

⁶ Especially see Hamid Algar, *Mirza Malkum Khan* (Berkeley and Los Angeles, 1973); Nikki R. Keddie, *Sayyid Jamal ad-Din "al-Afghani"* (Berkeley and Los Angeles, 1972); and Mangol Bayat, *Mysticism and Dissent: Socioreligious Thought in Qajar Iran* (Syracuse, N.Y., 1982). Among the important primary works and analyses of Qajar reformers in Persian are Fereidun Adamiyyat, *Fekr-e Azadi* (Tehran, A.H. 1340 [1961]); Mirza Aqa Khan Kermani et al., *Hasht Behesht* (n.p., n.d.); Nazem al-Islam Kermani, *Tarikh-e bidari-ye Iranian* (Tehran, A.H. 1332 [1953]); Ibrahim Safa'i, *Rahbaran-e Mashruteh* (Tehran, A.H. 1344 [1965–66]); Sefatallah Jamali Asadabadi, ed., *Maqalat-e Jamaliyyeh* (Tehran, A.H. 1312 [1933–34]); Iraj Afshar and Asghar Mahdavi, *Majmu'eh-ye asnad va madarek-e chap nashodeh dar bareh-ye Sayyed Jamal ad-Din mashhur be Afghani* (Tehran, 1963); and Mohammad Mohit Tabataba'i, *Majmu'eh-ye asar-e Mirza Malkum Khan* (Tehran, A.H. 1327 [1948–49]).

⁷ Nikki R. Keddie, *Religion and Rebellion in Iran: The Tobacco Protest of 1891–1892* (London, 1966), and the Persian, French, Russian, and English sources cited therein.

foreigners in Iran have had in comparison with those in many other countries of the Middle East and North Africa, Iranians, along with Afghans and a few others, have been more resistant to foreign domination than have most other peoples. Resistance has often been less obvious or militant in Iran than it has in Afghanistan, since in Iran periods of external accommodation to foreigners have alternated with periods of active rebellion. But Iranian antforeign feeling has always been strong.

Among the territories subjugated in the original wave of Muslim conquest, Iran was the only large area that retained its own language and a great deal of its old culture, albeit considerably modified by Islam. Iran's state religion since 1501, Shi'i Islam appears to have been even more resistant to foreign influences than Sunni Islam. (If the comparison is extended further, both main branches of Islam seem to have been more resistant to Westernization than have the dominant religions and traditions of non-Muslim Asia and Africa; thus Iranian Shi'i Islam may be the most resistant of all non-European religions to European culture.) Part of Shi'i Islam's strength in this regard lies in its insistence on ritual purity—including prohibiting physical contact with nonbelievers, preventing nonbelievers from entering mosques and shrines, and the like. Throughout the nineteenth century certainly, and for many Iranians much longer than that, the growing economic, political, and ideological influence of Westerners was perceived largely as the usurpation of the rights of believers. Economic, political, and religious resentments were thus intertwined, although different groups tended to stress different types of grievances. Governments seen as complaisant to foreign unbelievers were considered almost as culpable as the foreigners themselves. Not just in 1891 but in the constitutional revolution of 1905–11, the oil nationalization of 1951–53 under Mosaddeq, the demonstrations of 1963 around Khomeini, and the revolution of 1978–79 Iranians held their government responsible for Western depredations.⁸ A similar theme has certainly been sounded elsewhere, most notably among the Muslim Brethren and other Muslim militant groups in Egypt, Pakistan, and the Arab East, but in Iran the question of government accountability has attracted a wider and more revolutionary following. Attacks on any regime that permitted Western involvement in Iran have been strongly voiced by respected representatives of the orthodox ulama and the bazaar, which partly accounts for Iranian fervor. The strength of Iranian revulsion to foreign influence also arose from the long-held belief that Western nonbelievers were out to undermine Iran and Islam, and Shi'i Islam's encouragement of self-sacrifice to combat enemies has certainly added to resistance to foreigners based on exploitation and domination. For many, Shi'ism and nationalism were part of a single blend.

THE TWO TWENTIETH-CENTURY IRANIAN MOVEMENTS that clearly merit the title "revolution"—the "constitutional" revolution of 1905–11 and the "Islamic" revolution of 1978–79—demonstrate the importance of this Iranian outlook. In part, the events preceding the first Iranian revolution in this century were merely a continuation and intensification of the tobacco rebellion of the 1890s. The

⁸ Richard W. Cottam, *Nationalism in Iran* (2d edn., Pittsburgh, 1979); and Keddie, *Roots of Revolution*.

economic and political power of Britain and Russia grew rapidly after 1892. The tobacco "victory" saddled Iran with a £500,000 payment to the British tobacco company in compensation for its lost monopoly. On May 1, 1896, Mirza Reza Kermani, instigated by the antishah, pan-Islamic activities of Sayyid Jamal ed-Din "al-Afghani" and his Iranian and Shi'i circle in Istanbul, assassinated Naser ed-Din Shah. The shah's weak successor squandered far more money on courtiers and extravagant trips abroad than had his father. The son obtained the necessary monies from two Russian loans, granted on the basis of further Russian economic concessions. The British, not to be outdistanced by Russia in the race for profit in the Middle East, retaliated by requiring further concessions, chiefly the D'Arcy oil concession, which resulted in the first significant exploitation of Middle Eastern oil (following its discovery in 1908).

The Russo-Japanese War of 1904–05 and the Russian revolution of 1905 gave impetus to an Iranian opposition movement that had been growing since 1901. After a century of successive defeats, an Asian power had defeated a European power, an event that bolstered pride throughout Asia. This feeling was particularly strong in those countries, like Iran, that had experienced Russian penetration and oppression. Many considered it significant that the only Asian power with a constitution had defeated the only Western power without one, and constitutions came to be looked upon as the "secret of strength" of Western governments. In Iran, as in a number of Asian countries, treatises explaining constitutions and their virtues began to circulate, and news of Japanese victories was happily and rapidly spread. The Russian revolution demonstrated the possibility, at least in its first stage, of a mass revolt weakening a despotic monarchy and forcing it to adopt a constitution. Both the Russo-Japanese War and the Russian revolution also effectively, if temporarily, took Russia out of Iranian internal politics, an important consideration to those who expected Russia to intervene if the power of the Qajars was threatened or weakened.⁹

The constitutional revolution began late in 1905, when respected sugar merchants raised the price of the commodity in the face of rising international prices. The merchants were bastinadoed, and a rebellion broke out in the streets. When some of the ulama took sanctuary (*bast*), the shah promised a "house of justice" and other concessions. But the promise was not fulfilled, and a new rebellion broke out in 1906, highlighted by a new ulama *bast* in Qom and a *bast* by roughly twelve thousand bazaaris at the British legation. The crown then promised to accept a constitution, and a parliament was soon elected. The constitution of 1906–07 was modeled on the Belgian constitution, with one major exception. The Iranian constitution provided for a committee of five or more mujtahids to pass on the compatibility of all laws with the Islamic *sharia*, although this provision was never enforced. The framers intended real power to reside in the parliament and its ministers, rather than in the crown, but parliament was rarely able to wrest power from the shah.

⁹ The change in Iranian attitudes at this time is clear in documents pertaining to Iran in the British Foreign Office. Also see Nikki R. Keddie, "Religion and Irreligion in Early Iranian Nationalism," in Keddie, ed., *Iran: Religion, Politics, and Society* (London, 1980), 13–52.

The revolution became stormy and violent when a new shah, Mohammad Ali, closed parliament by a coup in 1908. Revolutionary guerrillas (*fedayin* and *mujahedin*) held out against the crown, first in Tabriz and later in Gilan, and then marched south to take Tehran along with Bakhtiari tribesmen moving up from the south. The second constitutional period saw a split between the moderate party, led by clerics, and the democrats, who had a program of agrarian and social reform. But the British and the Russians provided the revolution's *coup de grâce* in late 1911. The Russians presented an ultimatum demanding, among other things, that the Iranians get rid of their pronationalist American adviser, Morgan Shuster. The British, who had signed an entente with Russia in 1907, went along with the Russian demands, Russian and British troops moved in during 1911–12, and parliament was closed.¹⁰

Although parliament passed some social, judicial, and educational reform measures, the revolution was chiefly political, aimed at reducing monarchical and foreign power through the introduction of a Western-style constitution and parliament. This instrument and this body were seen as the best means to limit Iranian autocracy. Some of the revolution's participants expected Iran and its people to be able to return to more Islamic ways (like the barber who said he would shave no more beards, now that Iran had a constitution, since shaving was non-Islamic), while a smaller number hoped to become more Western, if only to be strong enough to escape Western control. In this revolution, unlike that of 1978–79, ulama leaders did not adopt a new political ideology. Those who supported the revolution were content to occupy a high proportion of the positions in parliament and to have a veto over legislation. One of the ulama wrote a treatise defending constitutionalism as the best government possible in the absence of the Hidden Imam, but there is no evidence that it was widely read.¹¹ Many of the ulama accepted the constitution as a means both to limit the shah's power and to increase their own; some became disillusioned by secularist laws and trends and quit oppositional politics.

Just as the revolution of 1905–11 followed smaller “rehearsals”—the movement against the all-encompassing concession to Baron Julius de Reuter in 1872 and the tobacco protest of 1891—so the revolution of 1978–79 built on resentments and organizations that surfaced in earlier protests and movements. The immediate

¹⁰ The Persian literature on this revolution is enormous. It includes invaluable classics by Nazem al-Islam Kermani, Ahmad Kasravi, Mehdi Malekzadeh, and Sayyed Hasan Taqizadeh as well as major background works by Fereidun Adamiyyat and Homa Nateq. For the main books in English, see Edward G. Browne, *The Persian Revolution of 1905–1909* (Cambridge, 1910); and Robert A. McDaniel, *The Shuster Mission and the Persian Constitutional Revolution* (Minneapolis, 1974). Other Persian works I have found useful in studying the constitutional revolution include Amin ad-Dauleh, *Khaterat-e siyasi*, ed. Hafez Farman-Farmaian (Tehran, 1962); Abolhasan Bozorgomid, *Az mast ke bar mast* (n.p., n.d.); “Haidar Khan Amu Oghli,” *Yadgar* 3 (A.H. 1325 [1946–47]): 61–80; and Mehdi Qoli Hedayat Mokhber as-Saltaneh, *Khaterat va khaterat* (Tehran, A.H. 1329 [1950–51]).

¹¹ The defense of constitutionalism was written in 1909 by Ayatollah Na'ini. Although it is discussed by H. Algar, among others, I have seen no Persian or Western books that refer to it before its republication with an introduction by Ayatollah Taleqani in 1955; see Algar, “The Oppositional Role of the Ulama in Twentieth-Century Iran,” in Nikki R. Keddie, ed., *Scholars, Saints, and Sufis* (Berkeley and Los Angeles, 1972). Na'ini apparently withdrew the book from circulation shortly after its publication; see Abdul Hadi Hairi, *Shi'ism and Constitutionalism in Iran* (Leiden, 1977), 124, 158.

post-World War II period was marked by the rise of leftist organizations, especially the Tudeh party, whose many strikes included a general strike in the oil fields, and of autonomist movements in Azerbaijan and Kurdistan, which expressed in considerable part genuine local sentiment. Then came the oil nationalization movement, which demonstrated deep anti-imperialist feeling and culminated in the nationalization of oil in 1951 and the two-year prime ministership of Mosaddeq. He was overthrown with the aid of the United States and Great Britain. Last in the series was the economic and political crisis of 1960–64, highlighted by demonstrations in 1963 that resulted in many deaths and brought about the exile of the religious leader of the movement, Ayatollah Khomeini, in 1964.

Pahlavi rule reversed the Qajars' policies on modernization and the development of the military, and after 1925 Iran was subjected to accelerated modernization, secularization, and centralization. Especially after 1961, the crown encouraged the rapid growth of consumer-goods industries, pushed the acquisition of armaments even beyond what Iran's growing oil-rich budgets could stand, and instituted agrarian reforms that emphasized government control and investment in large, mechanized farms. Displaced peasants and tribespeople fled to the cities, where they formed a discontented subproletariat. People were torn from ancestral ways, the gap between the rich and the poor grew, corruption was rampant and well known, and the secret police, with its arbitrary arrests and use of torture, turned Iranians of all levels against the regime. And the presence and heavy influence of foreigners provided major, further aggravation.

Ironically, the OPEC oil price rise of 1973 that the shah helped engineer was one cause of his undoing. He insisted on using the oil money for radical increases in investment and armaments that the economy could not bear: Iran faced galloping inflation, shortages of housing and consumer goods, and an increase of rural to urban migration that compounded the other problems. In addition, Iran became economically overcommitted as oil income fell after 1975. To cool the economy, the shah appointed Jamshid Amuzegar prime minister in 1977, but steps Amuzegar took to bring down inflation brought more hardship and discontent. A major cutback in construction, already in decline since early 1976, brought massive unemployment, which especially affected recent urban migrants, and a reduction in payments to the ulama increased the discontent of this influential class. In late 1977, partly emboldened by statements by Amnesty International, the International Confederation of Jurists, and President Jimmy Carter, Iranian intellectuals and professionals began to circulate petitions and letters calling for an extension of democratic rights.¹² A large educated and student class and a newly politicized class of urban poor, aided and influenced by the mosque network, provided the backbone for a new mass politics.

Early in 1978, the semi-official paper *Ettela'at* published an inspired and scurrilous assault on Khomeini, who was then attacking the regime from Iraq.

¹² The economic and political events of the 1970s are well covered in F. Halliday, *Iran: Dictatorship and Development* (Harmondsworth, Middlesex, 1979); R. Graham, *Iran: The Illusion of Power* (rev. edn., London, 1979); and Keddie, *Roots of Revolution*, chap 7.

Demonstrations with casualties ensued. Thereafter, on the traditional forty-day mourning intervals, demonstrations recurred, and religious, liberal, and leftist forces gradually coalesced against the regime. Khomeini went to France, where he could easily communicate with revolutionary leaders in Iran; the liberal National Front leadership reached an accord with him; and the shah's concessions were too few and came too late. The shah's gesture of appointing Shahpour Bakhtiar as prime minister led to Bakhtiar's expulsion from the National Front. Bakhtiar was unable to prevent Khomeini's return to Iran. And the ayatollah had become, even for many secularists, the symbolic revolutionary leader. In February 1979, air force technicians, supported especially by the Marxist guerrilla Fedayan-e Khalq and Muslim leftist guerrilla Mojahedin-e Khalq, took power for the revolutionaries in Tehran, and Khomeini's appointed prime minister, Mehdi Bazargan, took office.

Thenceforth, at least until 1983, the revolution moved ever more toward Khomeini's brand of absolutist religious radicalism. First, the National Front ministers resigned. Then, when U.S. embassy personnel were taken hostage by young "followers of Khomeini's line" on November 4, 1979, Bazargan and his foreign minister, Ibrahim Yazdi, were forced to resign in the face of their inability to obtain the hostages' release. Khomeini's choice for president, Abolhasan Bani Sadr, kept his post longer, but with decreasing power, and he was ousted in June 1981. Khomeini's Islamic Republican party came overwhelmingly into control of the cabinet and parliament. Once the party achieved a virtual monopoly on government, however, it lost cohesion, and increasing rumblings have been heard of internal disagreements—dissension within the ruling groups—on such issues as further land reform, personal power, foreign policy, and succession to Khomeini's position as the holder of *velayat-e faqih* ("guardianship of the jurisprudent"). While Khomeini-type religious radicals were first in the ascendant, in early 1983 conservatives became more powerful and blocked measures for land reform and a monopoly on foreign trade. Bazaar and other middle-class influences appeared to be growing, and there were moves toward political normalization and central control over religious radicals.

AMONG THE THEORIES OF REVOLUTION that shed light on Iran's two major upheavals in this century are James C. Davies's J-curve theory of revolution and Crane Brinton's *Anatomy of Revolution*. Davies suggests that revolutions emerge after a considerable period of economic growth followed by a shorter, sharp period of economic contraction and decline. C.-E. Labrousse had already described the economic improvements followed by a sharp downswing that preceded the French Revolution.¹³ Davies's J-curve matches the prerevolutionary experience of Iran in the 1970s. To a lesser degree, the revolution of 1905–11 may also fit the model, since some scholars have a period of growth in the late nineteenth century followed

¹³ See Davies, "Toward a Theory of Revolution," in Davies, ed., *When Men Revolt and Why* (New York, 1971), 137–47; and Labrousse, *La Crise de l'économie française à la fin de l'ancien régime et au début de la révolution* (Paris, 1944), introduction.

by economic difficulties that stemmed from the shah's extravagance and Russia's economic and political troubles after 1904.

Apart from Davies's model, the comparative pattern that best fits the revolution of 1978–79 is Brinton's more descriptive than explanatory typology.¹⁴ The political, economic, and financial troubles of an *ancien régime* that made rule in the old way impossible and forced accommodation with new groups were clearly seen both in the lesser crises that preceded the revolution and in the revolution of 1978–79. Such crises, in somewhat different form, were especially characteristic of the financial situation before the English (seventeenth century) and French (eighteenth century) revolutions discussed by Brinton. And political alienation of the intellectuals and the elite, including government figures, from the court was as characteristic of Iran in the 1970s as it was of Russia in the early twentieth century. The gradual and somewhat unexpected movement from demonstration to revolution, characteristic of Brinton's revolutions, has also characterized both Iranian revolutions. As late as the summer of 1978, after many major demonstrations and riots, most Iranian intellectuals voiced the view that the movement was over, having achieved its goal of liberalization with the shah's promises, especially of free elections, and many persons close to the Khomeini wing of the movement have insisted that he and his followers did not expect the shah to be ousted anytime soon.

The Iranian revolution of 1978–79 does not conform exactly to the pattern of growing radicalization fundamental to all four of Brinton's revolutions. To locate Khomeini on a right-left scale is not as easy as it may seem. On the one hand, he is a fundamentalist, a believer in a literalist application of scripture (except when it does not suit him); on the other, he is not only a fierce anti-imperialist, with particular dislike for encroachments by the United States and Israel, but also a man with concern for the poverty-stricken, a concern that has been manifested in such programs as free urban housing, state-supplied utilities, and further land reform,

¹⁴ James A. Bill has noted the correspondence between Brinton's views and the events of 1978–79; see Bill, "Power and Religion in Revolutionary Iran," *Middle East Journal*, 36 (1982): 22–47, esp. 30. The closeness of this fit is apparent in Brinton's own summary of the patterns apparent in the four great revolutions he discussed: "First, these were all societies on the whole on the upgrade economically before the revolution came, and the revolutionary movements seem to originate in the discontents of not unprosperous people who feel restraint, cramp, annoyance, rather than downright crushing oppression. . . . Second, we find in our prerevolutionary society definite and indeed very bitter class antagonisms. . . . Fourth, the governmental machinery is clearly inefficient, . . . partly because new conditions . . .—specifically conditions attendant on economic expansion and the growth of new monied classes, new ways of transportation, new business methods— . . . laid an intolerable strain on governmental machinery adapted to simpler, more primitive conditions. Fifth, . . . many individuals of the old ruling class . . . come to distrust themselves, or lose faith in the traditions and habits of their class, grow intellectual, humanitarian, or go over to the attacking groups. . . . [T]he ruling class becomes politically inept. . . . [I]t is almost safe to say that no government is likely to be overthrown from within its territory until it loses the ability to make adequate use of its military and police powers. That loss of ability may show itself in the actual desertion of soldiers and police to the revolutionists, or in the stupidity with which the government manages its soldiers and police, or in both ways. . . . [W]ith the attainment of power it is clear that [the revolutionists] are not united. The group which dominates these first stages we call the moderates, though to emotional supporters of the old regime they look most immoderate. . . . [A]t the crisis period the extreme radicals, the complete revolutionists, are in power. . . . This pervasiveness of the Reign of Terror in the crisis period is partly explicable in terms of the pressure of war necessities and of economic struggles as well as of other variables: but it must probably also be explained as in part the manifestation of an effort to achieve intensely moral and religious ends here on earth. . . . A striking uniformity . . . is their asceticism or . . . condemnation of what we may call the minor as well as the major vices." Brinton, *The Anatomy of Revolution* (Vintage Books, rev. edn., London, 1965), 250–51, 550.

despite their scant success. Perhaps “populist” is the closest political adjective—with the simultaneous leftist and rightist characteristics and xenophobic and sometimes fundamentalist components that that word connotes in American history. Populist rebellions that have appealed to the subproletariat in the West have sometimes turned into autocratic and even fascist movements, and some Iranians and Americans would say that this change has occurred, or is occurring, in Iran.

Brinton, in his typology of revolution, posited the fall of the radical element during a Thermidor, in which most people, overtaxed by the rule of virtue and justice, long for more accustomed, laxer ways. This deradicalization is in turn often followed by autocratic, usually military rule; in France Napoleon succeeded the Directory, and in Russia Stalin replaced the NEP. Neither of these stages has occurred in Iran as of early 1983, but both are possibilities. Indeed, the early phases of Thermidor may be discernible; Iran has taken major steps toward normalizing its economic and political relations with ideologically divergent regimes—notably Turkey, Pakistan, and some Western and Eastern European countries. And, although much of Iran’s internal and external policy has not softened as of March 1983, the growth in strength of the conservative faction in government and Khomeini’s December 1982 decrees providing some protection for legal rights may be early signs of a Thermidor.

With the rise of social history, socioeconomic explanations of revolutions have become more general than Brinton’s phenomenological comparisons. Although its revolution of 1978–79 can be explained in terms of socioeconomic causation (as I have attempted to do above and elsewhere), Iran fits less neatly into most existing socioeconomic comparative schemes than it does into the basic J-curve or more varied Brinton typology. The closest socioeconomic revolutionary model for Iran’s experience appears to be the Marxist formula, without any of the elaborations or modifications added recently.¹⁵ This formula, in essence, postulates that revolution occurs whenever the relations of production—particularly the control and ownership of the society’s basic means of production—have changed beyond the ability of the old forms of political power and state organization to subsume the new economic order. This situation essentially obtained prior to both Iranian revolutions.

During the revolution of 1905–11 the majority of economically dominant groups and classes—the growing and often thriving class of big and medium merchants, the landlords, particularly those engaged in growing cash crops, and the tribal

¹⁵ Marx and Engels’ basic view is stated, with some variations, in several works from the *Communist Manifesto* onward. Recent theoretical works touching on comparative revolution and influenced to some degree by Marx include those by Theda Skocpol, Charles Tilly, Eric Hobsbawm, George Rudé, and Barrington Moore. Although these shed much light on such topics as “primitive rebels” (Hobsbawm), the autonomy of the state (Skocpol), the nature of urban and rural rebellious crowds (Rudé), and the reasons why some societies have had major revolutions and others not (Moore), they have less to say than has Marx on the kind of forces that led to revolution in Iran. Naturally, examining the Iranian revolution is not their aim, but certainly more work now needs to be done by social historians and sociologists to produce general hypotheses or theories that will encompass the Iranian phenomenon, especially since it may not be the last of its type. Skocpol has modified some of her views in the aftermath of the Iranian revolution; see Skocpol, “Rentier State and Shi’a Islam in the Iranian Revolution,” *Theory and Society*, 11 (1982): 265–83, with comments by Egbal Ahmad, Nikki R. Keddie, and Walter L. Goldfrank, *ibid.*, 285–304.

khans—were decreasingly represented by the Qajars. The crown made few attempts to make laws and create conditions under which trade could flourish or to build up the state so as to be able to limit foreign control. At the same time the Qajars, unlike some other Middle Eastern rulers, had no strategy for increasing the loyalty of the ulama; instead, the crown added to the causes of ulama disaffection while allowing their independent power to grow. Although Iran did not yet have a strong bourgeoisie in the modern sense, groups whose interests lay in rationalizing the economy, encouraging trade and manufacture, and decreasing foreign control, were growing in size and influence. But the last Qajar shahs tended to squander the state's funds on luxurious living and foreign travel for court favorites and members of the royal family without foreseeing the disastrous financial consequences.

In the revolution of 1978–79 the conflict between major classes and the autocracy is even clearer. The reversal of Qajar policy toward modernization helped create a sizable, well-educated stratum of society, most of the members of which became bureaucrats and technocrats; others from this stratum entered the professions and arts or private industry. Many industrialists also sprang from humble origins in the bazaar. In addition to the workers' and subproletariat's grievances over the growing privileges not only of foreigners but of the rich as well, Iran's *nouveau riche*—that is, the relatively privileged new middle and upper classes and rich bazaaris—were discontented. Their economic futures were often determined arbitrarily and irrationally by fiat from the top, while they were denied all real participation in self-government and the political process. Both the successes and the failures of modernization put different classes, from the urban poor to the new middle classes, at odds with the autocratic government. And such contradictions were also felt by national minorities, which were economically oppressed and denied their own languages and cultures.

These various disaffections coalesced in two main ideological strains that already existed in embryo in the revolution of 1905–11: the liberal or leftist desire for Westernization, and the fundamentalist wish to return to a "pure" Islam, particularly as interpreted by Ayatollah Khomeini and those around him. The latter won out—hence the appellation "Islamic revolution"—but the grievances behind the revolution were at least as much socioeconomic as cultural.¹⁶

TO COMPARE THE REVOLUTIONS of 1905–11 and 1978–79 to each other can contribute as much to our understanding of Iran in the twentieth century as to compare them jointly to theories and paradigms developed by Western scholars. Although many points of similarity and difference can be noted, the most striking point of comparison may be stated as an apparent paradox: the constitutional revolution, despite the leading role played by many of the ulama, resulted in an

¹⁶ For an important comparative discussion of the revolution of 1978–79, see Gary Sick's forthcoming book. He has covered some of the same points in preliminary fashion in a recent article; see Sick, "Washington's Encounter with the Iranian Revolution," in Nikki R. Keddie and Eric Hooglund, eds., *The Iranian Revolution and the Islamic Republic* (Washington, 1982), 127–31, and his transcribed remarks in the ensuing "General Discussion," *ibid.*, 155–68.

almost wholly Western-style constitution and form of government, while the revolution of 1978–79, in which secular leftists and liberals—in addition to religious forces—played a major role, resulted in a self-styled Islamic republic and a constitution stressing Islam. This is not, moreover, simply a matter of constitutions. The revolution of 1905–11 was clearly secularizing in a number of spheres of life and law, while that of 1978–79 was just as clearly Islamicizing, despite the far greater Westernization of education, law, government, culture, and the economy that taken place by then. It is true that the revolutionaries in both the first and eighth decades of this century were fighting against autocracy, for greater democracy, and for constitutionalism, so that there was much ideological continuity. Nonetheless, the ideology of the revolutionary leaders was quite different in the two revolutions. Why?

The answer to this question lies largely in the nature of the enemy perceived by each group of revolutionaries. In 1905–11 the revolutionaries were fighting against a traditionalist regime and a shah whose dynasty had made very few efforts at Westernization, including reforms of potential benefit to Iran. At the beginning of the century, therefore, non-ulama reformers in particular, but also some liberal members of the ulama, found it easy to believe that the politically and economically encroaching West, which was obviously more powerful than Iran, could only be combatted if some Western ways were imitated. Nineteenth-century reformers called for Western-style armies, legal reforms, a clearly organized cabinet system, and modern economic development. For Muslims, many such ideas were justified by appropriate interpretations of the Quran and Muslim Traditions favoring change, acquiring knowledge from outside Islam, and taking the steps necessary to oppose unbelievers. When constitutions became a matter of interest after the Russo-Japanese war, the idea of adopting a Western-style constitution in order to limit autocracy and achieve the secret of Western strength gained greatly. Nor was this a ridiculous notion. Most Iranians were not yet ready to overthrow the Qajars, but many wanted to curb their arbitrary power; a constitution seemed—and under better circumstances might have been—a good way to accomplish their relatively limited objectives.

Both parts of the Iranian constitution (which lasted until 1979), the Fundamental Law of 1906 and the Supplementary Fundamental Law of 1907, were largely derived from the Belgian constitution. The clear intent was to have a constitutional monarchy of very limited power, a prime minister and his cabinet (appointments to which required the approval of parliament), and guarantees of freedom of speech, the press, and other civil rights. The provision for an appointed senate, half of whose members were to be designated by the crown as a conservative counterweight to the *majlis* ("lower house"), was not put into effect until 1950, and a key provision insisted upon by ulama leaders—for a committee of at least five leading members of the ulama to pass on the compatibility of *majlis* legislation with Islam—was never enforced, for reasons that have never been adequately explained. Perhaps the ulama under the leadership of Sayyed Abdollah Behbehani already so dominated the first *majlis* that the ulama no longer feared the passage of secular laws contrary to religious doctrine, or perhaps Behbehani secretly opposed

enforcing the provision because it might have reduced his own power. In any event, revolutionary leaders in 1905–11 found their model in Western-style liberalism and constitutionalism, and many of the ulama at least permitted the adoption of the constitution. Others broke away as the implications of secularization became clearer, and many tried to block certain aspects of Westernization.¹⁷ The growth of new bourgeois forces and of secularist ideas continued for many decades after 1911.

In 1978–79, however, the perceived enemy had changed, and the Iranian response was correspondingly different. For fifty years the Pahlavi dynasty had forced the Westernization of Iran. In the course of that Westernization the customs and beliefs as well as the prerogatives not only of the ulama but of many bazaaris and ordinary peasants, nomads, and the urban poor were attacked. Far more than the Qajars, the Pahlavis were perceived as tools of Western or Westernized powers, chiefly the United States and Israel. No longer could Iranians accept strong armies, Western-style industries, and modern legal codes and educational systems as solutions, in themselves, to Iran's problems. Even the liberal constitution had been subject to autocratic manipulation. The regime came to be seen as, among other things, too Western, and there developed among the alienated a search for roots and for a return to "authentic" Iranian or Islamic values. The nationalism that had read modern, liberal virtues into pre-Islamic Iran—expounded by intellectuals like the nineteenth-century Mirza Aqa Khan Kermani and the twentieth-century Ahmad Kasravi—had been largely coopted by the Pahlavi shahs. The shahs promoted pre-Islamic motifs in their speeches and in their architectural styles, and Mohammad Reza even sponsored a wasteful mythomaniacal celebration of a fictitious twenty-five hundredth anniversary of the Iranian monarchy and abortively changed the starting date of Iran's calendar from Mohammad's *hijra* to the foundation of the pre-Islamic monarchy.

Although many educated Iranians clung to their own liberal or leftist versions of this pre-Islamic and Western form of nationalism, some important intellectuals by the early 1960s began to turn to new ideas. In a famous essay, Jalal Al-e Ahmad attacked such "Westoxication," suggesting that Iranians look rather to their own and Oriental ways. Later he tried to rediscover Islam for himself, although his critical account of his pilgrimage makes it doubtful that he succeeded. Clerical and lay religious opposition grew at the same time, and some intellectuals published new essays and republished with new introductions works by religious reformers like Jamal ed-Din "al-Afghani" and the early twentieth-century Ayatollah Na'ini, who had in 1909 written the first reasoned clerical defense of a Western-style

¹⁷ The role of the ulama in the revolution of 1905–11 has become a point of controversy, in part because scholars have let their interpretation of history reflect their view of ulama action since the early 1960s. In 1969 Hamid Algar stated even more strongly than previous authors the general view of the ulama's importance and progressivism then, but Said Amir Arjomand in various articles and Willem M. Floor in a recent re-evaluation have played down the progressive role of the ulama in 1905–11 as in other periods. See Algar, *Religion and State in Iran, 1785–1906* (Berkeley and Los Angeles, 1969), esp. chap. 14; Arjomand, "The Ulama's Traditionalist Opposition to Parliamentarianism, 1907–1909," *Middle East Studies*, 17 (1981): 174–90, and "The State and Khomeini's Islamic Order," *Iranian Studies*, 13 (1980): 147–64; Floor, "The Revolutionary Character of the Iranian Ulama: Wishful Thinking or Reality?" *International Journal of Middle East Studies*, 12 (1980): 501–24; and Keddie, *Religion and Politics*, 73–98.

constitution as the best protection against autocracy. The important guerrilla group, the Mojahedin-e Khalq, combined new interpretations of Islam with socialist ideas that were often close to those of the great orator and hero of progressivist Islamic revolutionaries, Ali Shariati (d. 1977).¹⁸ Significantly, none of these groups or individuals should properly be termed "fundamentalist" or even fully "traditionalist." Most merely wished to escape the related evils of internal despotism and of "Westoxication"—socioeconomic and cultural dependence on the West.

Increasing numbers of Iranians shifted to progressive versions of an indigenous Islamic ideology perceived as likely to restore Iranian self-esteem and combat Westernization. Such versions were possible because so many liberal and even leftist ideals were contained in different strands of the Islamic revival. Liberal ideals were perhaps best represented by Mehdi Bazargan and, in a more traditionalist way, by Ayatollah Shariatmadari, both of whom were important in the revolution. The more progressive interpretations of Islam came from the very popular Ayatollah Taleqani and from Shariati, and leftist ones chiefly from the Mojahedin. Many continued as late as 1978–79 to advocate enforcement of the constitution of 1906–07, although they stressed the necessity of implementing its provision for a committee comprised of five or more of the ulama to ensure the compatibility of laws with the *sharia*.

Even the interpretations of the Ayatollah Khomeini, which in the end largely won out, were not, despite their partially "fundamentalist" emphasis on scriptural morality and punishments, really traditional. They contained new ideological elements appropriate to an Islamic revolution and to direct rule by the ulama. Khomeini's notion of direct ulama rule is new to Shi'ism, as not only Western scholars like myself but also a Muslim supporter of Khomeini has noted:

[T]he *mujtahids* were to some extent inhibited by the doctrine of the inherent illegitimacy of political authority in the absence of the Imam. The *usuli* position did not consider the possibility of the emergence of one of the *mujtahids* as the *mujtahid* of the *mujtahids*, as the Imam himself. And this confusion was there when Imam Khomeini returned to Tehran from Paris during the Revolution. For a time he was called "Naib-i-Imam." It was only gradually that the word "naib" was dropped and he became Imam Khomeini. Of course, the title of Imam does not mean that he is the twelfth Imam. It simply means that he is the leader of the *Ummah* at this moment. In other words, this is a new doctrine something akin to "first among equals." . . . [W]hile [people] can follow other *mujtahids* in religious matters, they can also follow Imam Khomeini as the political leader of the Islamic State, of the Revolution, indeed of the *Ummah* today. So, the Revolution has in a sense taken the Shi'ia political thought further. An actual practical difficulty has created a situation where it is possible for one of the *mujtahids* to become Imam of the *Ummah*. And this, of course, is a great advance on the earlier position of the Shi'ia political thought.¹⁹

¹⁸ On these intellectuals, see Yann Richard, "Contemporary Shi'i Thought," chapter 8 part 2 of Keddie, *Roots of Revolution*; and Keddie, *Roots of Revolution*, chapter 8 part 1: "Intellectual and Literary Trends to 1960." Among the most important works of these intellectuals, which give an idea of their criticism of the "Westoxication" of many Iranians and suggest the alternative Islamic reforms that some of them put forth, are Jalal Al-e Ahmad, *Gharbzadegi* (Tehran, A.H., 1341 [1961–62]), and *Dar khedmat va khianat-e raushanfekran* (Tehran, n.d.); Abolhassan Bani Sadr, *Eqtesad-e tauhidi* (n.p., n.d.); Ali Shariati, *Tashayyo'-e 'Alavi va Tashayyo'-e Safavi* (n.p., A.H. 1352 [1973?]), and *Islamshenasi* (n.p., n.d.); and Ayatollah Mahmud Taleqani, *Islam va Malekiyyat dar moqayeseh be nezamha-ye eqtesadi-ye gharb* (n.p., n.d.).

¹⁹ Kalim Siddiqui [sic, Siddiqi] et al., *The Islamic Revolution: Achievements, Obstacles, and Goals* (London, 1980), 16–17.

The victory of Khomeini's more absolutist version of Islam, with the addition to existing doctrines of ulama power the notion of direct rule, did not come because most people really preferred this to the more latitudinarian or progressive version of other clerical and lay Islamic thinkers but because, as a corollary to his doctrinal absolutism (as well as his charisma and leadership qualities), Khomeini was the most uncompromising opponent of the Pahlavis, of the monarchy itself, and of foreign control and cultural domination.

There is some convergence, not wholly accidental, between the "Manichean" world outlook of Khomeini and other Muslim thinkers and the more widespread phenomenon of "Third Worldism." The Manichean trend sees the world as largely divided into the just Muslim oppressed and the Western or Western-tied oppressors, and the more general ideology of the Third World similarly sees itself as economically drained and culturally colonized by an imperialist West. Such perceptions of "we" and "they," the Third World and the West, evince little appreciation of internal problems and class and other contradictions within either culture. Shariati, Bani Sadr, Ghotbzadeh, and others were directly influenced by varieties of Third Worldism, including the sophisticated version of Marxist dependency theory espoused by Paul Vieille, long Bani Sadr's friend and mentor, and the ideas of Frantz Fanon, whom Shariati admired. Khomeini himself has probably not been immune to such currents. At least one student of Khomeini's thought has noted the ayatollah's brand of Third Worldism and tentatively attributed it to contacts with Palestinians in Iraq, since such arguments entered his talks and writings during his exile there.²⁰ The fusion of "modern," secular Manicheanism, "traditional" Islam, and uncompromising hostility to monarchy, dependence, and imperialism created a revolutionary ideology that distinguished the revolutionaries from the Western and Westernized oppressors as much as the constitutionalist ideology of 1906 distinguished revolutionaries from traditional, nonmodernizing autocrats.

The blend of Islam and Third Worldism fits an anti-Western, anti-imperialist mood, particularly among students and those sections of the urban population who—in contrast to the minority of Westernized Iranians with Western-style, usually well-paying jobs in the government or private sector—were either poor or in the traditional economy. The revolutionaries in 1905–11 disliked Russian and British encroachments, but their main wrath was directed against the Qajar dynasty and its inability to organize a strong and functioning state and nation. Even though the main wrath of the revolutionaries in 1978–79 was apparently also directed against a dynasty and a shah, the cases are not really parallel. The late shah, with whatever degree of accuracy, was seen as a willing tool of the West, whose culture

²⁰ Gregory Rose, "Velayat-e Faqih and the Recovery of Islamic Identity in the Thought of Ayatollah Khomeini," in Nikki R. Keddie, ed., *Religion and Politics in Iran* (New Haven, 1973), 166–88. Also see Nikki R. Keddie, "Islamic Revival as Third Worldism," in J.-P. Digard, ed., *Le Cuisinier et le philosophe: Hommage à Maxime Rodinson* (Paris, 1982), 275–81. Many of Khomeini's speeches are available in rough translations from the Foreign Broadcast Information Service, and some of his works are translated in Hamid Algar, ed., *Islam and Revolution: Writings and Declarations of Imam Khomeini* (Berkeley, 1981). Not yet translated is the very important early book by Khomeini, *Kashf-e Asrar* (n.p., n.d. [ca. 1944]), in which he attacked both the Pahlavi monarchy and secularist intellectuals but did not yet completely reject monarchy or call for total rule by clerics.

and economic control had pervaded Iran in a way far more offensive to most than was the case in 1905. Iranians associated things Western with their plight, and they thought their cultural and economic problems could only be solved by a return to what they saw as purely Islamic ways. Hence, the paradox of a more "traditionalist" Islamic, more "antimodern" reaction in the revolution of 1978–79 than in that of 1905–11 can be explained primarily as a reaction to the rapid, exploitative growth of Western influence, of Westernizing rulers, and of new forms of imperialism in the intervening period.

THE DISILLUSIONMENT WITH THE WEST and the desire to return to Islam is not unique to Iran; it is found in different forms throughout much, if not most, of the Muslim world, sometimes in revolutionary ideologies and movements. These movements do not reject modern technology but call for a return to Islamic traditions, which are interpreted differently by reformists and fundamentalists. Often, as in Iran, enthusiasm for ideas like constitutionalism and republicanism continues, but in an Islamic context. The independent power, wealth, and ideological claims of the Shi'i ulama probably allowed its members to become the first leaders of a twentieth-century Islamic revolution. The unique strength of the Shi'i ulama and their consequent revolutionary leadership does not, however, negate the possibility of related revolts and revolutions in non-Shi'i lands. Similar conditions could provide the foundation and the desire for revolt, possibly intensified by the Iranian example and military success much as the Russo-Japanese War and Russian revolution in 1905 contributed to the Iranian constitutional revolution of 1905–11. The importance of the organized network of socioideological ties between the Iranian ulama and Iran's urban residents bears some resemblance to the urban organization of new Muslim groups in Egypt and elsewhere, despite lesser participation by the ulama in non-Iranian movements.²¹ And the widespread feeling that Western liberalism and socialism have been tried and found wanting is also important.

The changes wrought by the Iranian revolution have not run their course. Most Iranians who fought for an Islamic republic (and many were fighting rather for secular versions of justice, democracy, or even socialism) wanted a well-functioning, egalitarian state—the kind of government suggested by Shariati and by Khomeini in 1978. The growth of open discontent and its suppression in a series of small civil wars and rampant executions since the revolutionary victory is evidence that many, and probably most, were not fighting for what they got. What they did in fact get includes economic decline and upheaval, strict laws and rules (often "Islamic" at most in the sense that literalist Quranic and other legal punishments have sometimes been enforced in Muslim countries), arbitrary trials and summary

²¹ On the importance of Shi'i urban networks to the organization and fulfillment of the Iranian revolution, especially see Skocpol, "Rentier State and Shi'a Islam." On the economic and intellectual forces in contemporary Egypt, especially see Malcolm H. Kerr and El Saved Yassin, eds., *Rich and Poor States in the Middle East: Egypt and the New Arab Order* (Boulder, Colo., 1982); and Fouad Ajami, *The Arab Predicament* (New York, 1981).

punishments (not Islamic in any sense), a scramble for wealth and power among some of the ulama, new restrictions on women, and the like. Granted, the revolution had its more positive aspects: some concern for the poor and for equalization of incomes as well as limited kinds of local self-government and self management. Some Iranians also point to the decline in U.S. influence as a benefit of revolution, but how much this decline is a real advance in the absence of a well-functioning economy and a united country is questionable. The current balance has not proved satisfactory to most Iranians, although the mullas have shown themselves far more politically organized and capable of holding onto power than many, both within and outside Iran, would have expected.



Review Essay
Tales of Two Cities:
Florence and Venice in the Renaissance

GENE BRUCKER

SINCE WORLD WAR II, the historiography of Italian city-states in the late medieval and Renaissance periods (ca. 1350 to ca. 1550) has been dominated by foreigners, particularly by English and American historians. There is no simple explanation for this remarkable fact, unparalleled for any other European country. No obvious connection can be drawn between the tradition of prewar scholarship, which was predominantly German and Italian, and the Anglo-American monopoly since 1945. The significant expansion in fellowship support (Fulbright, the British Council) for young scholars was certainly a stimulus, as were too, for some, their wartime experiences in Mediterranean regions. While foreigners were increasingly attracted to the study of Italy's remote past, Italian historians were preoccupied with their contemporary history. After decades of research on the Cinquecento, Federico Chabod devoted the postwar years to a study of Italian foreign policy in the nineteenth century. This shift was symptomatic of the scholarly priorities of a generation of Italian scholars, who saw little that was relevant to their concerns in the struggles of Guelfs and Ghibellines, or even in the peninsula's occupation by foreign powers after 1494.¹

BY ESTABLISHING SCHOLARLY STANDARDS OF EXCELLENCE and creating a firm documentary foundation for the institutional history of Medicean Florence, Rubinstein has earned a secure place in the pantheon of Florentine historians. A cluster of recent books by younger scholars has expanded the areas of solid archival research and, through sensitive probing of the evidence, has deepened our understanding of the fundamental problems of urban experience in Florence and Venice. In their choice of subjects, their sources and methodology, their work might be described as "traditional," but that appellation is misleading. To some degree, they are all revisionist in their orientation, re-examining old problems with new evidence and insights and developing original conceptual frameworks for

¹ See Randolph Starn, "Florentine Renaissance Studies," *Bibliothèque d'humanisme et Renaissance*, 32 (1970): 677–84; and Gene Brucker, *The Civic World of Early Renaissance Florence* (Princeton, 1977), 3–13.

understanding Italian politics and society in the late medieval and Renaissance periods.

Nearly every important city-state in northern and central Italy has found its foreign historian, who has exploited local archives and libraries and has written a solid and informative history, based largely on unpublished records.² Most of these histories have focused on political developments in these towns during the communal era, and in the transition from commune to *signoria*. There are few studies of demographic and economic development; even rarer are works of regional scope or those that attempt comparative analysis. But the most striking feature of this historiography is its heavy concentration on Florence and Venice.³ During the 1970s, twenty monographs on Florentine late medieval and Renaissance history were published in English, while not a single book in that language was devoted to the history of Milan. And, although Rome has attracted a handful of Anglo-American historians of the postwar era, it has not received the scholarly attention that it deserves. I will concentrate on recent historical work by foreign scholars on the political, social, and economic history of Florence and Venice. This historiography has extended beyond the study of political developments (still the worthy objective of much scholarship on other towns) to an exploration of related problems—some heretofore explored only haphazardly, others not at all.⁴

In this large and diverse body of historical work, it is difficult to discern any conceptual or methodological pattern. No historian of Florence and Venice, native or foreign, has ever established a “school” or a tradition. Consequently, the scholarship on these cities has appeared to lack focus and coherence. Undoubtedly, its most distinctive feature has been its solid base in archival sources. Historians attracted to the study of Florence and Venice have luxuriated in the richness of their archives, whose contents and organization have strongly influenced the direction of their research. Public records, catalogued serially, have been more accessible and exploitable than private or ecclesiastical sources, which has contributed to the strong emphasis on political and institutional history. This postwar historiography, dominated by *stranieri*, has been empirical in its methods, positivist in its epistemology, and, until quite recently, untouched by the ideological struggles that, since 1945, have convulsed Italian historical writing.

The most eminent living historian of Florence is Nicolai Rubinstein, whose work exemplifies the very best of this Anglo-American scholarship. Rubinstein has labored in the Florentine archives since the 1930s; he was acquainted with the city’s

² For recent examples, see Christine Meek, *The Commune of Lucca under Pisan Rule, 1342–1369* (Cambridge, Mass., 1980); and William Bowsky, *A Medieval Italian Commune: Siena under the Nine, 1287–1355* (Berkeley and Los Angeles, 1981).

³ This concentration was already visible a decade ago in the multi-authored volumes edited by Nicolai Rubinstein and John R. Hale: Rubinstein, ed., *Florentine Studies: Politics and Society in Renaissance Florence* (London, 1968); and Hale, ed., *Renaissance Venice* (London, 1973).

⁴ On the dearth of monographs on Renaissance cities by Italian scholars, see Sergio Bertelli’s Introduction to Gene Brucker, *Firenze nel Rinascimento* (Florence, 1980), xiv. Two recent works deserve mention in this context: Guidobaldo Guidi, *Il Governo della Città-Repubblica di Firenze del primo Quattrocento*, 3 vols. (Florence, 1981); and Mario Bernocchi, *Le Monete della Repubblica fiorentina*, 4 vols. (Florence, 1974–78). Though lacking a sustained analytical dimension, they are valuable contributions to Florentine institutional history. See Judith Brown, Review of Guidi’s *Il Governo della Città-Repubblica di Firenze* in the *AHR*, 87 (1982): 1418–19.

great historians—Robert Davidsohn, Nicola Ottokar, Gaetano Salvemini—of an earlier generation. He has been an archival scholar *par excellence*; no one has ever matched his knowledge of the sources for the Florentine Quattrocento. Like Frederic Lane, the distinguished *maestro* of Venetian historiography, he has been committed to the principle of “the irreplaceable value of painstaking work and of honest and exact scholarship.”⁵ Although he has written on other cities (Siena) and other subjects (political theory), he has concentrated on the political and institutional history of Florence from the thirteenth to the early sixteenth century. His major work, *The Government of Florence under the Medici, 1434–94* (1966), was described by Philip Jones as “nothing less . . . than the true beginning of the systematic history of Quattrocento Florence.” With other reviewers, Jones praised the patience, pertinacity, and meticulousness of Rubinstein’s research “on the forbidding complexities of the Florentine constitution and the devices by which . . . it was manipulated for partisan domination.”⁶ The author described the methods by which the Medici and their allies maintained their authority in a republican polity. Although Rubinstein’s focus throughout is upon the techniques of government, his book also illuminates the complex relationship between political power and social status in Medicean Florence.

The laudatory reviews of this book are surely merited. Yet, while it established scholarly standards against which to measure all future publication in the field, the work also raised certain questions about methodology and conceptualization that, expressed cautiously and hesitantly in reviews, have lately been stated more openly and forcefully in evaluations of Anglo-American scholarship. Immediately noticeable in the first pages of *The Government of Florence* is the absence of any background or context, which suggests that the book was written primarily for specialists. Rare are the references to earlier scholarship, and none to any historiographical tradition in which the book might be located. In his brief preface, Rubinstein pointed to the fundamental differences between the Medici regime with its preservation of republican institutions and other despotic governments in fifteenth-century Italy. But he is not interested in any comparative analysis of political systems and would surely have agreed with Burckhardt’s dictum that neither Florence nor Venice “can be compared to anything else which the world has hitherto produced.” Rubinstein’s concentrated focus on “who governed and how they were chosen”⁷ contributes to the clarity of his argument, but it does leave unexplored and unexplained other important issues: the structure of the Medicean party; the actual administration of the state; the evolution of political ideology. The manipulation of electoral systems tells us much about power and its uses, but not, perhaps, the most important things. Finally, there is the matter of the author’s empathy with his subjects, the ruling class of Medicean Florence. Some critics have suggested that foreign historians of Florence and Venice have identified too closely

⁵ Fernand Braudel, Foreword to *Venice and History: The Collected Papers of Frederic C. Lane* (Baltimore, 1966), xii.

⁶ Jones, Review of Rubinstein’s *The Government of Florence* in *History*, 52 (1967): 319.

⁷ Daniel M. Bueno de Mesquita, Review of Rubinstein’s *The Government of Florence* in the *English Historical Review*, 83 (1968): 121.

with their elites, thereby losing detachment and objectivity and, in particular, rejecting any consideration of alternate systems of government.⁸ To this largely unconscious bias, a Florentine historian has recently alluded, in describing his determination “to look beyond the aura of inevitability that historians tend to give to established authority.”⁹

A good example is John Najemy's *Corporation and Consensus in Florentine Electoral Politics, 1280–1400* (1982). Najemy identified the procedures for electing civic officeholders as a key issue in Florentine politics in the late thirteenth and fourteenth centuries. By focusing sharply on this single issue, he was able to describe and clarify the patterns of political conflict within the commune and to identify “two competing conceptions of communal society and politics” (page 3), the corporate and the consensual. Too ambiguous to be defined as ideologies, these rival conceptions are continuous threads running through Florentine politics for this century-long period. Phases of intense conflict were followed by periods of stasis, reflecting particular historical circumstances and the fluctuating strength of guildsmen, who favored a corporate policy, and their oligarchic opponents. Although the corporate model was ultimately defeated by its enemies, Najemy has insisted upon the vitality and durability of this guild-based conception of politics and of its pervasive influence (the notions of equality, fraternity, consultation) on Florentine republicanism. Based upon exhaustive research and a close scrutiny of the documents, Najemy's analysis is very persuasive and, in many of its parts, seemingly impervious to challenge or correction. More consciously than most scholars who have studied this period, Najemy has sought to integrate his analysis into the historiography of Italian (and European) urban history: problems of social structure and social conflict; the relationship between political and socioeconomic change, and between political experience and civic ideology. Perhaps the most vulnerable part of his argument is his formulation of the “consensus” model, with its suggestion of unity and cohesiveness within the city's elite.

In *The Rise of the Medici: Faction in Florence, 1426–1434* (1978), Dale Kent has studied the emergence of a Medicean faction in the late 1420s and its ultimate triumph over a rival, and more inchoate, party led by prominent oligarchs from the Albizzi and Peruzzi families. Her analysis of these factions and their struggle for dominance is the most thorough and solidly documented investigation of “party” for any Italian city-state in the communal and signorial periods. In the private correspondence of the Medici, Kent found the evidence for identifying the partisans, whose place in Florence's status hierarchy she determined from supplementary evidence—electoral, notarial, and tax records. Though never so informative as to provide full answers, these letters do furnish clues to the complex motivations that impelled Florentines to join one or the other faction. Kent decoded and analyzed this evidence with consummate skill. Not all of the mysteries

⁸ For examples of this criticism, see Renato Pecchioli, *Studi veneziani*, 13 (1971): 693–708; Eric Cochrane and Julius Kirshner, “Review Article—Deconstructing Lane's Venice,” *Journal of Modern History*, 47 (1975): 321–34; and Anthony Molho, in *Italia e Stati Uniti d'America: Concordanze e dissonanze* (Rome, 1981), 201–44.

⁹ Randolph Starn, *Contrary Commonwealth: The Theme of Exile in Medieval and Renaissance Italy* (Berkeley and Los Angeles, 1982), xv.

surrounding the Medici's ascendancy are fully resolved by her analysis; the role of their wealth, for example, is still problematic (pages 29–30). But her book establishes a solid documentary foundation, and provides a model, for future research on the Medici party and its political success.¹⁰

Supplementing the public records (legislation, debates, electoral and tax sources) and the chronicles, these private sources have enabled Kent to construct a richer and more nuanced picture of Florentine political life in this critical juncture than is possible for any other Italian government. Recent studies of Venetian politics are, by comparison, less solidly documented and more tentative in their conclusions. The pervasive influence of the *mito di Venezia*, which denied the existence of factional discord within the city's ruling class, has contributed to the image of a stable and durable regime, governed by men united in their political interests and in their dedication to the republic's welfare. But scholars who, in recent years, have searched for evidence of conflict, have often been thwarted by the sources, which reveal so little about the ways in which power was exercised and political decisions were made in Venice. That obscure world may eventually be illuminated through intensive archival research, although the impediments are truly formidable. Instead of plunging into that documentary morass, Robert Finlay decided to focus his study, *Politics in Renaissance Venice* (1980), on private records, most notably the fifty-eight published volumes of Marino Sanuto's diaries, covering the years 1496 to 1533.

In his analysis, Finlay confronted the political problems raised by the myth. How did this polity achieve so remarkable a degree of stability and continuity? How did it avoid the factionalism that was the curse of other republican regimes? Finlay assumes that Venetian nobles were no more virtuous, and not necessarily more civic-minded, than their neighbors. The key to political stability, he argued, was the creative use of electoral competition to involve the majority of the nobility in the political system and, at the same time, to discourage the formation of factions. "Hostility within the patriciate was channeled into place seeking and elections. Passions were expressed by excluding others from office, with ballots, not with blood" (page 219). The political system was thus stabilized by means that other ages would describe as corrupt. As the diarist Priuli wrote, "In Venice there is nothing that cannot be obtained and nothing that cannot be forgotten with time, lobbying, and intrigue." Finlay's interpretation minimizes the role of ideology as a force in Venetian politics. He did not see much evidence of particular families forming power blocs in elections and in policymaking. Within the constitutional structure, he placed more emphasis on the role of the Great Council, and on the political influence of the doge, than have many historians. He also stressed the system's pragmatism and flexibility, its resilience during and after crises (for example, the War of the League of Cambrai), and its ability to change in response to events. Chapter 5, on the problem of dissent, is a superb analysis of the workings of this

¹⁰ Also see the evaluations of her work by Werner Gundersheimer in *Speculum*, 54 (1979): 822–26; John K. Hyde in the *English Historical Review*, 97 (1982): 177–78; Roberto Bizzocchi in the *Journal of Modern History*, 52 (1980): 339–41; and Samuel Cohn in the *Renaissance Quarterly*, 33 (1980): 70–72.

polity, which demonstrates Finlay's sensitive exploitation of evidence and his talent for using specific incidents to illuminate broad issues.

Not the least of the virtues of Finlay's book is his demonstration that the shell of Venetian "political silence" can be breached and that the personalities and careers of individuals—Luca Tron and Marino Sanuto, for example—can be described in some detail. Still, writing Venetian biography will never become a major industry since, for reasons that remain unclear, private records for Venice's Renaissance centuries are relatively sparse and uninformative. The Florentine advantage in this area is overwhelming; witness the current project to publish Lorenzo de' Medici's correspondence in ten volumes.¹¹ Two recent biographies of Bartolomeo Scala and Filippo Strozzi illustrate the genre, in its Florentine context.

Alison Brown's case study of Bartolomeo Scala, the obscure notary from Colle who became chancellor of the Florentine republic (1465–94), is a model of scholarly thoroughness. She has described Scala's public career, his "place in society" and his literary work in rich detail. Three themes in this biography focus on important, and controversial, problems in the current historiography of Medicean Florence: (1) patronage; (2) administrative reform; and (3) Laurentian culture. Scala came to Florence (in his own words) "naked, disadvantaged, of the lowest parentage . . . , absolutely penniless, without reputation, patrons, or kinsmen" (page 3). Influential patrons helped Scala in his meteoric rise, although there are disagreements over their identity and their methods. Brown argued that Scala's talent and ambition contributed substantially to his career and that the Medici were not his only patrons. The issue, not fully resolved by *Bartolomeo Scala, 1430–1497, Chancellor of Florence: The Humanist as Bureaucrat* (1979), appears to be, Was Medici support the exclusive and decisive route to significant career advancement in Florence? The most original and controversial part of this biography (chapters 6 and 7) concerns Scala's service as chancellor. Brown has depicted him as a dedicated reformer whose goal, a goal that she suggested was shared by Lorenzo, was the rationalization of the chancery as part of the broader transformation of the government into a prototype of the "modern state." Although each specific reform appears modest in scope, the whole program did represent a significant change in administrative technique and spirit. Brown also traced, in Scala's writings, the formulation of a theory of state sovereignty that foreshadows Machiavelli and Bodin. She viewed Lorenzo's regime as more creative and "progressive" than have most historians, who have stressed its traditional and patrimonial qualities.¹² Although the evidence to support her provocative thesis is sometimes thin and ambiguous, Brown has raised fundamental questions about the nature of government and the relationship between experience and ideas in Laurentian Florence.

Republican and despotic regimes, patrons and clients, the connections between wealth and political power: these were perennial problems in Florence, providing a degree of continuity and coherence to its history. Bartolomeo Scala rose from

¹¹ The exception is Paolo Sarpi, whose career has been intensively studied. See, too, Caetano Cozzi, *Il Doge Nicolò Contarini: Ricerche sul patriziato veneziano agli inizi del seicento* (Venice, 1958).

¹² Also see the reviews by Anthony Molho in the *Renaissance Quarterly*, 33 (1980): 420–22; Mark Phillips in the *Journal of Modern History*, 52 (1980): 344–46; and Julius Kirshner in *Speculum*, 55 (1980): 529–32.

obscurity to eminence under the Medici before being cashiered by the republican regime in 1494. Filippo Strozzi's career also had its dramatic vicissitudes, which are well described in Melissa Bullard's *Filippo Strozzi and the Medici: Favor and Finance in Sixteenth-Century Florence and Rome* (1980). Son of a wealthy banker who had been brought back from exile by the Medici in the 1460s, Filippo contracted a marriage (1508) with Clarice, granddaughter of Lorenzo de' Medici, at a time when his descendants were in exile. This marriage was the foundation of Filippo's fortunes, making him one of the richest and most powerful men in Italy during the pontificates of the Medici popes, Leo X and Clement VII. The patronage of those ecclesiastical princes was crucial to Filippo's career; from them he received the offices of depository of the Florentine Signoria and the papal curia. These posts allowed the banker to tap the revenues of both fiscal systems and to transfer Florentine public monies to Rome to pay for the wars of the Medici popes. Working with fragmentary archival materials from private and ecclesiastical sources, Bullard has pieced together the record of these complex fiscal transactions. But Filippo's life illustrates much more than the intricacies of Renaissance fiscal practice. He was the beneficiary, and ultimately the victim, of a patronage system controlled by the Medici; in a very real sense, he was a "creature" of that dynasty. When Clement VII died in 1534, Filippo's economic situation deteriorated. Throwing in his lot with the republican cause that he had previously ignored, Filippo led the anti-Medici army that was defeated at Montemurlo (1537). In the end, he was forced by events to play a political role as a citizen who, like his model Cato, was prepared to sacrifice his life for liberty. This is a first-rate monograph: lucid, concise, tightly organized, carefully and thoroughly documented, it is a significant contribution to Renaissance fiscal practices and to the patronage networks that controlled access to power and wealth.

In chapter 2, "Filippo Strozzi's Florence," Bullard summarized the current knowledge of the city's economic situation in these decades, particularly the fortunes of the aristocracy. The scholarly work in this area is very thin and spotty. Most intensively studied have been the structure and operations of companies engaged in commerce, banking, and cloth manufacturing. But generalizations about the Florentine economy as a whole, or any of its major sectors, are hazardous at this juncture. There are, however, encouraging signs of a sustained effort by economic historians to lay the foundations for a synthesis, based in part on statistical analysis, of the city's economic development.¹³

Hidetoshi Hoshino's study of the Florentine woolen cloth industry in the fourteenth and fifteenth centuries, *L'Arte della lana in Firenze nel basso medioevo: Il Commercio della lana e il mercato dei panni fiorentini nei secoli XIII–XV* (1980), is based largely on the surviving account books of *lanaiuoli*, supplemented by evidence from tax and guild records and from doganal documents in Italian towns that imported Florentine cloth. His conclusions, carefully and cautiously developed, significantly revised the older interpretations of Doren and Davidsohn, which have been

¹³ The Venetian economy has been more thoroughly studied than has Florence's; see Brian Pullen, ed., *Crisis and Change in the Venetian Economy in the Sixteenth and Seventeenth Centuries* (London, 1968); and the articles by Lane, Pullen, and Ugo Tucci in Hale, *Renaissance Venice*.

enshrined in the standard accounts of Florentine economic history. Hoshino has persuasively argued that the development of the woolen cloth industry was not synchronized with the rest of the city's economy. Not until the 1320s did Florentine *lanaiuoli* begin to produce substantial amounts of high-quality cloth, gradually replacing the fine Flemish and French products in the Mediterranean market. Villani's estimate of 75,000 cloths produced in 1338 was, Hoshino demonstrated, wildly exaggerated; the true figure was between 24,000 and 30,000. The woolen cloth industry was quite prosperous in the second half of the fourteenth century; it did not suffer from the collapse of the great banking firms in the 1340s or from the effects of the population decline after the Black Death. Hoshino traced the vicissitudes of the industry in the fifteenth century: a significant decline in the 1420s (11,000–12,000 pieces) followed by recovery after 1440. In the 1490s, the Levantine market for Florentine cloth was so strong that production rose to more than 20,000 pieces annually. In addition to its revision of production figures, Hoshino's *L'Arte della lana in Firenze* is particularly informative on the marketing and quality and price differentials of the cloth produced in Florentine *botteghe*. His focus is sharply defined and limited; some problems—labor, for example—are not addressed. But the book's strengths outweigh these limitations and provide a solid contribution to the scholarship on the Florentine woolen cloth industry.

Hoshino might be faulted for the narrowness of his focus and for his reluctance to integrate his research into a broader context. Those criticisms do not apply to Jean-Claude Hocquet's pioneering study of the role of salt in Venetian history. This two-volume work, *Le Sel et la fortune de Venise* (1978–79), combines quantitative research in the *Annales* tradition with a bold and original interpretive vision.¹⁴ The importance of salt for the Venetian economy has long been recognized, but heretofore its production and marketing have not been systematically studied. Hocquet has filled that lacuna, but he has done much more. He located the salt trade firmly within the context of Venetian history, relating it to state policy and to the shifting fortunes of Venice's ruling elite. In the first, "medieval" phase of this saga (to ca. 1280), Venetians intensively exploited their local salt works and also expanded and strengthened their market monopoly in northern Italy. During the crucial "Renaissance" phase, from the late thirteenth to the late sixteenth century, the Venetian government deliberately suppressed local production, replacing it with salt (used as ballast for its ships) from distant ports in the Levant and the Balearic isles. This policy, Hocquet argued, was designed to subsidize Venice's long-distance trade and thus to promote the welfare of its merchant class at the expense of consumers, who paid artificially high prices for their salt under monopoly conditions. Sometime around 1600, Venice's salt trade foundered, as did its shipping industry. Local needs were again satisfied by local production. This summation does scant justice to the complexity and sophistication of Hocquet's argument, which, from this sharp focus on one commodity, touches upon so many important aspects of Venetian experience. For some tastes, his analysis will be excessively schematic, materialist, and doctrinaire, but his wide-ranging interpreta-

¹⁴ Hocquet has summarized his conclusions in "Capitalisme marchand et classe marchand à Venise au temps de la Renaissance," *Annales: Économies, sociétés, civilisations*, 34 (1979): 279–304.

tion should stimulate a thorough re-evaluation of Venetian history during these crisis-filled centuries.¹⁵

In Hocquet's interpretation, Venetian salt policy was a classic illustration of an elite's ability to devise a strategy for its own economic and political advantage. Selfish and ruthless the policy may have been, but it was formulated and implemented with great skill. Richard Goldthwaite's *The Building of Renaissance Florence: An Economic and Social History* (1981), which focuses on the construction industry in the fifteenth century, is similarly concerned with the activities and motives of the Florentine elite, specifically its massive investment in private palace building. Goldthwaite described building practices and techniques, the costs of materials and labor, and the roles of patrons, architects, artisans, and laborers in this building boom. He has written the most detailed and comprehensive study of the construction industry for any European community (or region) in the preindustrial era. The chapters devoted to these economic and technical topics are of very high quality, fully meriting the accolades they have received from critics.¹⁶

In sharp contrast to Hocquet's scenario for Venice, where rich merchants profited at the expense of consumers and the poor, the fifteenth-century Florentine economy Goldthwaite described was robust. Artisans and workers benefited from the building boom subsidized by wealthy citizens. The private building sector was a model of free enterprise, largely unregulated by guilds or commune and sensitive to market conditions of supply and demand. Goldthwaite's analysis reflects his bias in favor of the private over the public sphere and his sympathy for a Burckhardian world of entrepreneurs and individualists, whose energy and vision created Renaissance civilization. Some critics have suggested that this picture is altogether too benign.¹⁷ Not all of Goldthwaite's generalizations are supported by adequate empirical data; indeed, some can be refuted or qualified by evidence from sources that he has exploited—the *catasto*, the *Mercanzia*, and the *Innocenti*. It has been argued that the economy was less buoyant, the tax burden heavier, wealth less equitably distributed, and artisans and workers less prosperous than Goldthwaite believed. Although his bold attempt to sketch a broad picture of Florence's material and social milieu may be premature, it will surely stimulate further research and inspire other scholars to formulate their syntheses of the Florentine Quattrocento.

IF THE WORKS DISCUSSED THUS FAR have sometimes employed techniques, concepts, and methods that derive from current historical practice, they nevertheless fit comfortably into the mainstream of Florentine and European historiography. They would not have startled, or puzzled, Salvemini or Davidsohn. Some recent books have been more strikingly innovative in conceptualization and methodology—and

¹⁵ For a detailed evaluation, see Alberto Tenenti, "Il Sale nella storia di Venezia," *Studi veneziani*, new ser., 4 (1980): 15–26.

¹⁶ Nicolai Rubinstein, Review of Goldthwaite's *The Building of Renaissance Florence* in the *Renaissance Quarterly*, 35 (1982): 274–78; Domenico Sella, *ibid.* in the *AHR*, 87 (1982): 211; Caroline Elam, *ibid.* in the *Times Literary Supplement*, November 13, 1981, p. 1335; and Felix Gilbert, *ibid.* in the *New York Review of Books*, January 21, 1982, pp. 65–66.

¹⁷ Gene Brucker, Review of Goldthwaite's *The Building of Renaissance Florence* in the *Journal of Modern History*, 53 (1981): 692–94; and Julius Kirschner, *ibid.* in the *Journal of Economic History*, 41 (1981): 671–72.

also more controversial. Quantifiers have been very active in European urban history, and their techniques have been adopted by several scholars working in Florentine and Venetian history. The second important influence has come from anthropology, whose concepts and vocabulary have been particularly influential in studies of social structure, civic ideology, and ritual.

Although Italian cities have generally preserved more ample records than their transalpine counterparts for the late medieval and Renaissance centuries (before 1600), this documentation is usually too fragmented and spotty to permit the construction of reliable secular trends for population, prices, wages, rents, and the like. Undaunted by these limitations, economic and demographic historians have searched for quantifiable data that will provide some solid evidence for their analyses. In 1976, Charles de la Roncière published his massive, four-volume study, *Florence centre économique régional au XIV^e siècle: Le Marché des denrées de première nécessité à Florence et dans sa campagne et les conditions de vie des salariés, 1320–1380*.¹⁸ He gleaned his data on the economy of Florence and its *contado* in the fourteenth century from a broad spectrum of published and unpublished sources, but his richest finds came from the records of the city's largest hospital, Santa Maria Nuova, and for the *contado* from a cluster of notarial records. In his first volume, he traced prices and salaries in the city from 1320 to 1380; volume 2 analyzes the commune's annona policy for these decades and the links between population trends, prices, and wages. His analysis of the economic relationships between city and *contado* in volume 3 is particularly valuable, since it treats themes—*contado* markets and entrepreneurs, for example—that had not previously been subjected to systematic study. In his conclusion, De la Roncière discussed the economic context of the Ciompi revolt and formulated an ingenious theory to explain that uprising. His historical vision is unusually broad in scope, embracing religious and ideological themes as well as the material conditions of fourteenth-century Florentines and their rural cousins. Future work on Florence's Trecento economy will be based on the foundations established by De la Roncière.

Judith Brown's monograph on the relationship between Florence and one of its subject towns, *In the Shadow of Florence: Provincial Society in Renaissance Pescia* (1982), traces the demographic and economic fortunes of Pescia over three centuries, from the Black Death to the seventeenth century. Her work is based on statistical evidence from tax records and other economic sources, supplemented by a careful reading of archival records in Florence and Pescia. The breadth and solidity of Brown's research lends credibility to her conclusions; her conceptual framework deserves praise for its concern with the larger issues of the economic and political integration of Tuscany in the early modern period. Her evidence does not support the traditional interpretation of Florence's subject territory as being exploited and impoverished by the capital city. Indeed, Pescia's fortunes improved significantly from the mid-fifteenth to the late sixteenth century as a result of lower taxes and of

¹⁸ De la Roncière summarized his views on the Ciompi in his "La Condition des salariés à Florence au XIV^e siècle," *Il Tumulto dei Ciompi: Un Momento di storia fiorentina ed europea* (Florence, 1981), 13–38. For an extension of De la Roncière's work on laborers' living standards, see Giuliano Pinto, "I livelli di vita dei salariati cittadini nel periodo successivo al tumulto dei Ciompi, 1378–1430," *ibid.*, 161–98.

the integration of the town's commercial and industrial activities into a Tuscan regional economy. Pescia's aristocracy also profited from its close relationship with the Medici grand dukes, who recruited its members for their administration and conferred honors and benefits upon them. Pescia was not the whole of Tuscany, and Pesciatine prosperity may have been exceptional. But Pescia's experience does suggest the need for study of regional diversity in Tuscany and for the abandonment of clichés like "imperialism" that do not help us understand the patterns of statebuilding and economic change in the early modern period.¹⁹

The computer made its debut in Florentine historiography with the publication of the collaborative work by David Herlihy and Christiane Klapisch-Zuber, *Les Toscans et leurs familles* (1978). In the late 1960s, these enterprising scholars began their assault upon the records of the *catasto* of 1427, which contains the statements of 60,000 heads of Tuscan households concerning their family members (260,000 souls) and their property. The main object of this study was the formation of a demographic profile of the people living under Florentine rule in 1427. Chapters 1 through 5 (describing the *catasto*, its origins and its implementation) are a valuable contribution to Florence's fiscal and administrative history in the early Quattrocento; the compilation of the *catasto* was an extraordinary administrative achievement, perhaps unparalleled in European history before the eighteenth century. The middle chapters trace population movements and patterns in Tuscany from 1300 to 1550 and formulate hypotheses concerning birth and death rates, marriage and fertility, the fluctuating size and structure of households over time. Chapter 9 describes the heavy concentration of wealth in Florence, as compared to the territory, and, within Florence, among a very small group of citizens, 1 percent of whom owned one-fourth of the city's resources and one-sixth of Tuscany's. The concluding section of this *capolavoro* discusses the complexities of the Tuscan family: its unstable and variable structure, and its economic, social, and emotional dimensions.

By any criterion, *Les Toscans et leurs familles* is the most significant contribution to the urban history of Renaissance Italy in recent years. Its utilization of the computer to organize vast amounts of demographic and economic data has been a boon to the field. The machine-readable edition of the *catasto*, prepared by Herlihy, Klapisch-Zuber, and their collaborators, is a precious scholarly resource.²⁰ The book's chapters are a treasure trove of information culled from the tax records and other archival sources (*libri dei morti*, notarial records, and the like), and from private diaries, letters, sermons, and theological tracts. By combining statistical data with material that is unquantifiable, the authors hoped to present a more rounded picture of the Tuscan family and its environment, but the integration of this evidence is not always successful. A degree of fragmentation and disjointedness in a multi-authored work is not surprising and perhaps explains the complaint that the

¹⁹ For a summary of recent scholarship on granducal Tuscany, see Furio Diaz, *Journal of Italian History*, 1 (1978): 95–110.

²⁰ For its value to other scholars, see, for example, Thomas Kuehn, *Emancipation in Late Medieval Florence* (1982), 208 n. 16.

authors have failed to develop a coherent view of their subject, which to one reviewer seemed more like a Picasso than a Masaccio painting. But these reservations do not seriously detract from the magnitude of this achievement; another reviewer has described *Les Toscans et leurs familles* as a work “che ... induce a riflettere sulla sacralità della storia e sul sacerdozio dello storico.”²¹

The most perceptive review of this book has been written by Francis William Kent,²² whose own work on Florentine patrician families complements this demographic study. Whereas Herlihy and Klapisch-Zuber strove for comprehensiveness in their analysis, Kent focused upon three lineages and, from his intensive exploration of their surviving records, has created a very solid yet nuanced portrait of the Florentine family in its many dimensions and manifestations. His conceptual vision has been influenced by his readings in anthropology, but his approach is quintessentially historical. He does not view Quattrocento Florence as just another Mediterranean village. Kent used *catasto* data for his analysis of the household, and his findings support the conclusions of Herlihy and Klapisch-Zuber that upper-class households varied widely in their composition and organization and, indeed, were in continual flux. He found no evidence of any seismic shift from extended to nuclear households in the fifteenth century. He has argued convincingly that Florentine patricians possessed strong feelings for their paternal lineage, which influenced their behavior and helped define their social and political obligations. Exploiting a wide range of sources both public and private, Kent has constructed the fullest and richest picture of family structure that exists for any European urban community in the Renaissance period. His *Household and Lineage in Renaissance Florence: The Family Life of the Capponi, Ginori, and Rucellai* (1977) has been both warmly praised and sharply criticized. His conclusions, it has been argued, are flawed by the small sample of lineages that he investigated and by his lack of analytical rigor. He has been faulted for neglecting the economic activities of his patricians, for treating them in isolation, and for ignoring or minimizing the pressures for change in family structure and sensibility.²³ These criticisms are not so much reasoned responses to Kent's analysis as they are expressions of dissatisfaction with his demolition of cherished beliefs about the nature of Florentine society. This book is a model worthy of emulation: exemplary in its conceptualization, in its sensitive interpretation of evidence, and in its stylistic elegance.

The legal aspects of family history have not been studied in depth; indeed, there has been very little integration of legal and social history for any community in medieval and Renaissance Italy. Legal historians have been preoccupied with theory while neglecting social experience; “social historians,” as Thomas Kuehn noted, “have been loath to bloody themselves with the law” (page 5), daunted by its complex language and terminology. In *Emancipation in Late Medieval Florence* (1982), Kuehn has made a modest but significant assault on the barriers dividing

²¹ Edward Muir, Review of Herlihy and Klapisch-Zuber's *Les Toscans et leurs familles* in the *Journal of Interdisciplinary History*, 11 (1981): 253; and Aldo de Maddalena, *ibid.* in the *Rivista storica italiana*, 92 (1980): 253.

²² Kent, Review of Herlihy and Klapisch-Zuber's *Les Toscans et leurs familles* in *Speculum*, 55 (1980): 129–31.

²³ James C. Davis, Review of Kent's *Household and Lineage in Renaissance Florence* in the *AHR*, 84 (1979): 1089–90; Lauro Martines, *ibid.* in the *Renaissance Quarterly*, 31 (1978): 198–99; George Holmes, *ibid.* in the *English Historical Review*, 96 (1981): 609–10; and Richard Goldthwaite, *ibid.* in *Speculum*, 53 (1978): 817–19.

those disciplines. He has immersed himself in the legal sources (codes, statutes, *consilia*) and the scholarly literature; from the *Mercanzia* and notarial records, he has culled thousands of cases of the emancipation of children by Florentine fathers, and, through statistical analysis, he has traced patterns of emancipation from the mid-fourteenth to the early sixteenth century. He has persuasively argued that the key to emancipation, for both parents and children, was not age but the legal obligations for support and the payment of debts. Looking beyond the legal dimensions of the emancipatory act, Kuehn sought to understand its implications for this community's values: the notions of honor and reputation, and perceptions of a proper relationship between parents and their offspring. There may have been, in the upper echelons of Florentine society, a progressive elaboration of family techniques for survival. In this well-researched and cogently argued monograph, Kuehn has clarified one important facet of that phenomenon and has also suggested other themes (the *fideicommissum*, inheritance, and marriage strategies) that should be studied in their legal and social contexts.²⁴

A recently published monograph by F. W. and D. V. Kent on the electoral district (*gonfalone*) of the Red Lion explores the social and political matrix of a Florentine neighborhood in the fifteenth century. The Kents have gained an intimate and unrivaled knowledge of Florentine society, with all of its complexities and peculiarities, in the Medici period. The Red Lion district was the home of the Rucellai and the Strozzi, who were important though not dominating elements in this parochial society. Among the active participants in the district's affairs (which included electoral and fiscal responsibilities) were merchants and bankers from lesser families, artisans and shopkeepers, and some poor and obscure workers in the cloth industry. Some of the most interesting pages in *Neighbours and Neighbourhoods in Renaissance Florence* (1982) are devoted to bonds that linked together the residents of the Red Lion and the purposes for which they were forged. The authors admitted that the emergence of the Medici did change the nature of patronage networks in Quattrocento Florence, but they have stressed the strength and durability of neighborhood ties and interests, which resisted the centralizing pressures emanating from the Palazzo Vecchio and the Medici palace. The importance of neighborhoods in Italian urban society has long been recognized; this book explores the topic in depth, and with an unusual degree of precision and concreteness.

The sources for Italian urban history are so heavily weighted toward "the rich, the well-born, and the powerful" that any attempt to describe the lives of the poor is to be welcomed. The studies of De la Roncière and Herlihy-Klapisch-Zuber have illuminated some of the dark corners of this lower-class world; Samuel Cohn has devoted a monograph to *The Laboring Classes of Renaissance Florence* (1980). The book's conceptual framework is derived in part from Marxism, in part from social science theory. Into this framework Cohn has introduced statistical techniques that

²⁴ In contrast to Florence, the study of Venetian family history is quite undeveloped. But see J. C. Davis, *A Venetian Family and Its Fortune, 1500–1900: The Donà and the Conservation of Their Wealth* (Philadelphia, 1975); and Stanley Chojnacki, "In Search of the Venetian Patriate: Families and Factions in the Fourteenth Century," in Hale, *Renaissance Venice*, 47–90.

heretofore have been employed mainly by historical demographers. He believes that Florentine history must be understood as, fundamentally, a history of class conflict, of a continuous struggle between rich and poor, between employers and workers. He believes, too, that the Ciompi Revolution of 1378 was a classic example of violent class struggle, comparable to the French Revolution. And he sought to explain why Florentine workers in the Trecento were so prone to revolt, so willing to challenge their masters, while in the next century they were so docile and passive. This change in worker attitudes and behavior resulted, so Cohn argued, from a major restructuring of their living and working patterns and also from the creation of a more powerful and centralized state apparatus. In Trecento Florence, people from all strata of society lived together in the same neighborhoods; such a milieu, Cohn has suggested, fostered city-wide contacts among workers, a precondition for labor revolt. In the fifteenth century, economic and social pressures broke up the traditional structures, creating working-class neighborhoods on the city's outskirts, while the center became the exclusive preserve of the aristocracy. Isolated in their ghettos, the workers were unable to forge and sustain the city-wide associations that had existed prior to 1378.

Cohn's interpretation of Florentine history contrasts sharply with the views of many scholars working in this field. He has seen classes where others have not (or, at least, not in the Marxist sense); he has stressed tensions and cleavages within the social order, where others have emphasized bonds linking the social strata.²⁵ The most innovative feature of his book is the attempt to apply statistical techniques to particular sources, to prove his theories about class formation and class consciousness. He has identified marriage contracts as the most significant form of social interaction and, from a sample of these documents, has extracted data on residence and dowries, from which (with the computer's aid) he has developed his argument. As he admitted, his sources are not consistent, and the evidence is not easily collated for computer analysis. These methodological problems raise doubts in this reader's mind about the reliability of his data and the validity of his conclusions. The statistical evidence seems too thin and uneven to bear the weight of his argument, particularly his notions about changes in the structure of residence patterns and the class consciousness of Florentine workers. If there is a lesson to be learned from Cohn's book and from his ingenious and imaginative methodology, it concerns the limits of quantitative analysis in exploring social relations and *mentalités*. A careful reading of sources may sometimes be more illuminating than counting.²⁶

Cohn's arguments concerning changes in Florentine patterns of criminal behavior, from Trecento to Quattrocento, are also impaired by the nature of his

²⁵ Cohn, "Rivolte popolari e classi sociali in Toscana nel Rinascimento," *Studi storici*, 20 (1979): 747–58; and Molho, *Italia e Stati Uniti d'America: Concordanze e dissonanze*, 201–44.

²⁶ For an alternative methodology for studying the Florentine working class, see G. Pinto, Review of Cohn's *Laboring Classes of Renaissance Florence* in the *Journal of Modern History*, 54 (1982): 593: "more minute investigations directed toward the recovery of the individual families of the lowest classes." I concur with Felix Gilbert's statement that Cohn's primary concern was not the living and working conditions of Florentine laborers but "the establishment of features common to the origin and development of revolutionary movements in urban Europe"; Gilbert, Review of Cohn's *Laboring Classes of Renaissance Florence* in the *New York Review of Books*, January 21, 1982, p. 65. For a trenchant evaluation of Cohn's methodology, see Lauro Martines, *ibid.* in *Speculum*, 57 (1982): 595–97.

evidence. The criminal court records—the sources of his statistical data—changed radically over this time span, so that comparisons have only a limited value. Yet, he is undoubtedly correct in his contention that the post-Ciampi regime developed more effective instruments of social control, which were aimed primarily though not exclusively at the lower orders. In his study of criminality in fourteenth-century Venice, Guido Ruggiero has relied upon statistical data for his argument and, like Cohn, was concerned with the social context of crime and punishment in this urban milieu. His *Violence in Early Renaissance Venice* (1980) begins with a discussion of the Venetian system of criminal justice; here Ruggiero described the methods and institutions of criminal investigation and the functioning of the magistracies responsible for judging and penalizing those convicted of crimes. From the records of these offices, Ruggiero has compiled data on particular crimes and their penalties. He was particularly interested in the incidence of criminality among the various groups in Venetian society, which he has dissected into five distinct categories. His evidence suggests that the nobility committed a much larger number of the criminal acts that he has counted (rape, assault, murder, “verbal violence”) than its proportion of the population (18 percent of the crimes from less than 5 percent of the population). This effort to analyze crime statistically in terms of social groups is inevitably hazardous, and Ruggiero candidly admitted the difficulties in the interpretation of his evidence.²⁷ He was on firmer ground in his analysis of penalties—what they reveal about the views of Venetian nobles concerning the relative seriousness of crimes and the use of state power to control social behavior. As reviewers have pointed out, Ruggiero’s monograph has conceptual and methodological flaws.²⁸ But it is an important pioneering work, conceived and written with few, if any, models to serve as guides in an unexplored field. Future students of crime and punishment in Renaissance Italy will profit from Ruggiero’s mistakes as well as from his achievements.

Ritual behavior in Italian urban society has recently attracted the attention of scholars who have been influenced by the work of anthropologists. Ronald Weissman’s *Ritual Brotherhood in Renaissance Florence* (1982), a study of confraternal experience from the fourteenth to the sixteenth century, is an example of this new interest. Confraternities as religious organizations were not his primary concern; rather, they are “a vehicle for examining the relationship between [Florentine] ritual behavior and social organization.” Weissman’s introductory section, “Judah the Florentine,” describes the range of social networks in Florence and the significance of these associations for the male participants. Weissman stressed the competitiveness and abrasiveness of this milieu, “the agonistic character of social relations” (page 41). In a society in which the individual needed as much support as he could muster, confraternities were an important source of that critical commod-

²⁷ For a discussion of the unreliability of crime statistics in preindustrial societies, see Bruce Lenman and Geoffrey Parker, “The State, the Community, and the Criminal Law in Early Modern Europe,” in V. Gatrell *et al.*, eds., *Crime and the Law: The Social History of Crime in Western Europe since 1500* (London, 1980), 11–48.

²⁸ Samuel Cohn, Review of Ruggiero’s *Violence in Early Renaissance Venice*, in the *Journal of Social History*, 15 (1981): 298–301; Brian Pullan, *ibid.* in the *Renaissance Quarterly*, 35 (1981): 384–86; Werner Gundersheimer, *ibid.* in the *AHR*, 86 (1981): 877–78; Thomas Kuehn, *ibid.* in the *Journal of Modern History*, 54 (1982): 133–35; and Robert Finlay, *ibid.* in the *Journal of Interdisciplinary History*, 13 (1982): 348–49.

ity. Weissman's research has focused on the confraternity of San Paolo, whose history he traced from its origins in 1434 to the mid-sixteenth century. He has done a computer-aided study of the confraternity's membership, exploring such problems as age, duration of participation, economic and social background, and officeholding, and he has supplemented his analysis with data from other confraternal statutes and deliberations and with material from tax records. This information is carefully compiled and evaluated, and the conclusions drawn from it are quite credible. Sensitive to the temporal dimension of his topic, Weissman has attempted to link changes in confraternal organizations and activity to social, political, and religious developments in Renaissance Florence from the Black Death to the Medici *principato*. After a fifty-year period of turmoil, inaugurated by the republican restoration of 1494, Florentine confraternities were stabilized and given a different set of religious and social functions under the watchful eye of the Medici grand dukes. This provocative and innovative study employs statistical analysis with commendable caution and develops an original thesis concerning Florentine social and religious organizations, parts of which will surely be tested and challenged by Weissman's fellow-workers in the Florentine vineyard.

Edward Muir's *Civic Ritual in Renaissance Florence* (1981) focuses upon the public rather than the private dimensions of social and political experience in the Adriatic city. Although his reading in anthropological literature contributed to the formation of his methodology, Muir remained sensitive to the historical, the particular and discrete, dimensions of his subject. In Part 1, Muir discussed the *mito di Venezia* and its historical treatment; this first part concludes with a consideration of problems facing the historian who studies myths "as guides to the inherited symbols and mentalities of a particular culture" (page 56). Part 2 describes specific examples of legends that originated in Venice's medieval (and poorly remembered) past and that, in the sixteenth century, were incorporated into civic ritual. Muir's final section examines the role of the Venetian state in promoting civic rituals for political ends and in determining their particular forms. His evidence is sometimes thin, but he has made a plausible case for his theory that local, parish-based ceremonials were gradually supplanted by city-wide, state-sponsored events controlled from the ducal palace. He has convincingly argued that during the sixteenth century Venetian public ritual achieved its most elaborate and symbolic phase. The importance, for Venice's ruling elite, of these rituals and the ideology that they articulated can scarcely be overemphasized. Muir's sensitive and well-written analysis of this important and neglected subject is a valuable contribution to Venetian historiography.²⁹

If caution and restraint in the interpretation of evidence are distinctive qualities of Muir's scholarship, then boldness of conception and an interpretive vision that can truly be called revolutionary are the salient features of Richard Trexler's remarkable book, *Public Life in Renaissance Florence* (1981). Several parts of his argument have appeared in article form, but in this book Trexler integrated these fragments into a sustained, coherent, and powerful vision. His subject is ritual

²⁹ See Richard Trexler's Review of Muir's *Civic Ritual in Renaissance Venice* in *Speculum*, 57 (1982): 642–46.

behavior, and he sought to demonstrate its crucial importance for understanding Florentine history. He rejected the conventional view of Renaissance Florentines as the dynamic and creative if contentious builders of a brilliant civilization, constantly struggling among themselves for economic advantage, social supremacy, and political power. Instead, for Trexler, Florentines were actors in a social drama, playing out their ritualized roles in every dimension of their lives, private as well as public. The first two parts of the book define and illustrate these ritual roles; the second two parts discuss the significant changes in ritual forms and experiences, from the traditional mode of the "classical commune" to the new style created by Lorenzo de' Medici and continued by Savonarola. In the communal age, civic rituals were organized by the city elders who wielded political power. Their primary concern in sponsoring and orchestrating ritual practices was to gain honor for themselves and their city in a feudal world that viewed them, their mercantile activities, and their community with condescension and contempt. Lorenzo, however, moved freely in the company of princes, and, under his aegis, Florence enjoyed greater respect and honor. He also transformed and expanded the ritual life of the city by encouraging the participation of young men who replaced their elders as the guardians of civic virtue. That trend was continued by Savonarola, who brought women and children of both sexes into the religious processions and ceremonies that were so prominent during his ministry. This was, in brief, the ritual revolution that transformed Florentine public life and laid the foundations for the city's metamorphosis from republic to *principato*.

Public Life in Renaissance Florence easily wins the award for the most controversial book on Italian urban experience. The radically revisionist nature of Trexler's argument will guarantee its hostile reception by some critics, who may be so incensed by the book's palpable flaws that they ignore its genuine merit.³⁰ The introductory chapters, in particular, are replete with obfuscatory language and overstatement; the second half of the book is, by contrast, better written and more cogently argued. Even at its most perverse and quixotic, Trexler's reading of the evidence is invariably stimulating and provocative. Although each part of his thesis will doubtless be challenged, his insistence upon the creative role of ritual in Italian urban life will surely leave its mark. Whether Trexler's professed objective, "to modernize the discourse among Renaissance historians" (page xxvi), will be realized is problematic, but his distinctive interpretation of Florentine history will be read and pondered by every student, present and future, in the field.

THIS OVERVIEW SHOULD CONVEY a sense of the vitality and ferment that characterize recent historical scholarship on Renaissance Florence and Venice. Much of this work is consciously innovative: in subject matter, in methodology, in interpretation. In some instances, the zeal for novel techniques and explanations may have taken

³⁰ For a distinctly negative reaction, see Felix Gilbert, Review of Trexler's *Public Life in Renaissance Florence* in the *New York Review of Books*, January 21, 1982, pp. 62–64. For more positive though critical evaluations, see Francis W. Kent, *ibid.* in the *Journal of Modern History*, 54 (1982): 382–88; and Randolph Starn, *ibid.* in the *Art Bulletin* (forthcoming).

some scholars beyond the limits of the credible (and certainly the provable), but these interpretations, however dubious, can serve a constructive purpose by forcing critics to examine their assumptions and clarify their views. This veritable explosion of scholarship has advanced our knowledge substantially, particularly of demographic and economic developments³¹ and in areas of religion and formal behavior. While the study of these urban societies has progressed apace, problems of social stratification, and of the relationship between social, political, and cultural change, are still unresolved. Indeed, it must be conceded that the results of much of this scholarly work are still unintegrated, and the prospects for any synthesis of urban experience in these cities seem very remote.³² The barriers to integration and synthesis are formidable, but they must be surmounted if this rich and fertile field is to prosper in the future.

³¹ Soon to be published is a computer-based analysis of data from the records of the *monte delle doti*, with important conclusions concerning mortality rates, marriage patterns, and dowries in Quattrocento Florence. See Anthony Molho and Julius Kirschner, "The Dowry Fund and the Marriage Market in Early Quattrocento Florence," *Journal of Modern History*, 50 (1978): 403–38.

³² But see Lauro Martines, *Power and Imagination: City-States in Renaissance Italy* (New York, 1979).

Review Essay
Is There a New International History
of the 1920s?

JON JACOBSON

SINCE THE APPEARANCE OF THE FIRST ARCHIVE-BASED WORKS on the post–World War I international order over fifteen years ago, the study of Europe during the 1920s has advanced significantly. Scholars have exploited a vast amount of source material from government, business, and personal archives in Germany, England, France, Belgium, and the United States: business records have been used for the study of international relations, foreign office documents have been employed for the evaluation of relations between government and business elites, and personal collections have added depth and insight to the more official sources.¹ The historical examination of international relations has expanded in scope from studies of national and bilateral foreign policy to international histories of Europe and the Atlantic region. What constitutes international history has been conceived anew to include the role of private and central banking in international politics, the politics and diplomacy of financial crisis, the effects of national rivalry on monetary stability, trade and finance as instruments of power politics, the impact of intergovernmental indebtedness on national security, and the influence of multinational cartels on international political stability.² All of this scholarly activity has resulted in nothing less than the emergence of a new international history of the 1920s—one that abstains from tired debates over the primacy of domestic or foreign policy, avoids the isolation of military and political relations from financial and economic concerns, and examines the interpenetration of the private and public sectors of the international political economy. This may not amount to the

¹ The vast extent of this literature is evident from the excellent “Guides to European Diplomatic History Research and Research Materials,” a series edited by Christoph M. Kimmich and published by Scholarly Resources, Inc., Wilmington, Delaware: George W. Baer, ed., *International Organizations, 1919–1945: A Guide to Research and Research Materials* (1981); Alan Cassels, ed., *Italian Foreign Policy, 1919–1945: A Guide to Research and Research Materials* (1981); Christoph M. Kimmich, ed., *German Foreign Policy, 1919–1945: A Guide to Research and Research Materials* (1981); Robert J. Young, ed., *French Foreign Policy, 1918–1945: A Guide to Research and Research Materials* (1981); and Sidney Aster, ed., *British Foreign Policy, 1918–1945: A Guide to Research and Research Materials* (forthcoming).

² These topics provided the subjects for discussion at two recent international conferences: “Politischer Nationalismus und weltwirtschaftliche Finanz- und Handelsordnung: Konstellationen internationaler Politik in den Jahren 1924–1931/32,” held in Dortmund, October 1981, organized by Gustav Schmidt; and “Inflation and Reconstruction in Germany and Europe, 1914–1924,” held in Berkeley, July–August 1982, organized by Gerald D. Feldman, Carl-Ludwig Holtferich, Gerhard A. Ritter, and Peter-Christian Witt.

"more valid total explanation of policy" called for by Charles S. Maier,³ but it does suggest that some of the elements of an integrated analysis for the study of international history are being identified.

Not only do many recent works incorporate these developments, but they share as well a common subject matter, the internal and international politics and economy of Europe during the first half of the 1920s, and discuss many of the same issues and events. They utilize some of the same sources and rely to a greater or lesser degree on recently accessible French government archives. Charles S. Maier amazes us, in *Recasting Bourgeois Europe: Stabilization in France, Germany, and Italy in the Decade after World War I* (1975), with his insight into three national societies, his model for comparative history, his original and compelling vision of the 1920s, and his synoptic reinterpretation of the history of the first half of the twentieth century.⁴ Heavy demands are, however, imposed on the reader by his novel categories of analysis and by his sometimes elusive abstractions juxtaposed with densely detailed discussions of interelite negotiations over issues that are both complex and remote from the more familiar events of high policy.

Stephen Schuker's elegantly conceived thesis, his well-controlled presentation, and his lucid style display both the exceptional merits and the limitations of his *The End of French Predominance in Europe: The Financial Crisis of 1924 and the Adoption of the Dawes Plan* (1976). His is the most massively documented of the works considered here, a model of the multiarchival approach to research. Examining in depth the domestic sources and international context of French foreign policy, he has used both financial constraints and personal character as explanatory tools within his thesis and included much fascinating detail, both technical and biographical. Schuker has raised some highly significant issues without, however, fully resolving them: Could the Treaty of Versailles have been implemented integrally, or was its revision inevitable? Did financial constraints or personal fallibilities impose the most important limitations on the success of French policy? Of the various sources of French financial duress, which were the most significant?

Jacques Bariéty's *Les Relations franco-allemandes après la première guerre mondiale* (1977) is a more bilateral history, but one that contains little national bias and displays full appreciation for the problems facing policy makers in both Paris and Berlin. Through lengthy descriptions of the course of negotiations, he has shown how interrelated were the issues and how complex were the strategies pursued not only by the French and the Germans but also by the British. Walter A. McDougall's provocative hypotheses and stimulating interpretation in *France's Rhineland Diplomacy, 1914–1924: The Last Bid for a Balance of Power in Europe* (1978) are important additions to what has been an uncontroversial field of historical study, and they merit consideration and debate. McDougall recognized that French policy makers responded to the agonizing dilemmas that confronted them with policies that were

³ Maier, "Marking Time: The Historiography of International Relations," in Michael Kammen, ed., *The Past before Us: Contemporary Historical Writing in the United States* (Ithaca, N.Y., 1980), 380.

⁴ Maier's synoptic reinterpretation, first set forth in *Recasting Bourgeois Europe*, has been formulated more fully in "AHR Forum—The Two Postwar Eras and the Conditions for Stability in Twentieth-Century Western Europe," *AHR*, 86 (1981): 327–52.

diverse and often incoherent. Rather than oversimplify this inconsistency of policy, he formulated it in his own paradoxical style.

La Question des dettes interalliées et la reconstruction de l'Europe, 1917–1929 (1978) by Denise Artaud makes clear that war debts were a central issue in the power relations between America and Europe and that international payments were the pivot on which postwar stability turned. Marc Trachtenberg has succeeded where few would have believed possible—in making the technicalities of reparations understandable and the politics of reparations interesting. The least comprehensive and most argumentative of the works treated here, *Reparations in World Politics: France and European Economic Diplomacy, 1916–1923* (1980) gains significance from Trachtenberg's willingness to draw out the full implications of the evidence, to face central issues directly, and to deal with their inherent complexities in an analytical manner. Dan P. Silverman has demonstrated how both the reparations and war debt questions interplayed with other issues of economic and financial reconstruction to dissolve the wartime alliance of France, Britain, and the United States. His *Reconstructing Europe after the Great War* (1982) represents a continuation of the effort—begun by the other works noted here—to combat the view that the greed, incompetence, and ignorance of French policy makers delayed postwar financial stabilization and to arrive at a new, less hostile understanding of French policy.

The examination of previously unused sources and the utilization of integrative analyses have led to revised interpretations. But no one has made clear the lines of conflict and areas of consensus within recent scholarship. Although specific points of disagreement and sometimes vehement debate are manifest in reviews and footnotes, no comprehensive analysis of differences of interpretation and no assessment of the current state of scholarship has yet appeared in print.⁵ And, if the implications of recent research for an understanding of the international history of the twentieth century are to be clarified, the issues that this literature as a whole raises must be systematically defined, the terms of debate need to be made explicit, and the results need to be synthesized and placed in a larger historical context.

THE HISTORY OF INTERNATIONAL RELATIONS during the 1920s has essentially been viewed from one of three perspectives. Each attempts to explain why the post-World War I order was so vulnerable and ephemeral and why lasting peaceful stability was not achieved. The first does so by distributing responsibility among national foreign policies, the second by identifying conflicting national interests, and the third by defining contradictions within the international system. Although these three perspectives differ from one another and represent sequential stages in

⁵ Debate has largely centered on the level of moderation and constructiveness to be found in French policy and especially on the feasibility of French reparations proposals. See Walter A. McDougall, "Political Economy versus National Sovereignty: French Structures for German Economic Integration after Versailles," and Marc Trachtenberg, "Reparation at the Paris Peace Conference," with Comments by Charles S. Maier, Klaus Schwabe, and Gordon Wright, *Journal of Modern History*, 51 (1979): 4–85; and Peter Krüger, "Das Reparationsproblem der Weimarer Republik in Fragwürdiger Sicht: Kritische Überlegungen zur neuesten Forschung," *Vierteljahrshefte für Zeitgeschichte*, 29 (1981): 21–47. Sally Marks's *The Illusion of Peace: International Relations in Europe, 1918–1933* (New York, 1976) appeared too early to synthesize the "new history."

the historiographical development of the field, they have not always been regarded as mutually exclusive; indeed, they continue to coexist within recent scholarship and sometimes within individual works.

The first of these perspectives is essentially moralistic and voluntaristic. It seeks to assign specific blame for the breakdown of the Versailles settlement. Either the Germans are held responsible because they did not fulfill the terms of the treaty, or the Allies are made responsible because they imposed a settlement that they then neither sufficiently enforced nor revised. McDougall has cleverly termed this the *Nachkriegsschuldfrage*—the “post-war-guilt question”⁶—because historical controversy over the origins of the First World War has been transferred to the 1920s. In both cases, understanding is limited to substantiating the combatants’ innocence or guilt for the war of 1914–18. Even though this was the first interpretation of the 1920s to emerge, the one originating out of the emotions of the time, its residues continue to be found in recent scholarship.

The second perspective, one informed by the realist school of political theory, also remains influential. It seeks to avoid the categorization of nations according to guilt and to emphasize instead the conflict of national interests within a competitive system of states, what Maier has called “the continued irreducible challenge of international rivalries.”⁷ Until the Dawes reparation settlement of 1924, Berlin resisted French attempts to maximize at German expense French military security, financial solvency, and industrial productivity. And, after the Locarno Treaties of 1925, Paris resisted Germany’s efforts to ensure its own security, solvency, and industrial requirements—a recovery of tariff sovereignty and an end to military occupation, reparations, and unilateral disarmament. German demands were expressed by moral slogans such as *Freiheit* and *Gleichberichtigung* just as earlier French demands were phrased as the “integral implementation of the treaty.” Behind these slogans lay issues of national capabilities and relations of power. Reparations could transfer real wealth from one country to another. The security of France was Germany’s insecurity; the security of Germany was Poland’s insecurity. Additional conflicts of interest existed among the victors of World War I and found expression in alternative strategies of stabilization. The French sought a static system of permanent guarantees against German recovery; the Anglo-Americans countenanced peaceful change and the reintegration of Germany. Until 1924, the British stalemated the French; from 1925 through the early 1930s, the French stalemated the British.

The third perspective, the one most closely associated with recent scholarship, views the lack of international stability during the 1920s as a function of fundamental contradictions within the international system.⁸ First, the require-

⁶ McDougall, Review of Hermann J. Rupieper’s *The Cuno Government and Reparations, 1922–1923: Politics and Economics* (The Hague, 1979) in the *Journal of Modern History*, 53 (1981): 363.

⁷ Maier, “Marking Time,” 386. For the classic statement of conflicting national interests within the interwar Anglo-French entente, see Arnold Wolfers, *Britain and France between Two Wars: Conflicting Strategies of Peace from Versailles to World War II* (New York, 1940).

⁸ Maier, *Recasting Bourgeois Europe*, chap. 4; McDougall, *France’s Rhineland Diplomacy, 1914–1924*, 3–5, 114, 128, 134, and “Political Economy versus National Sovereignty,” 8–9; and Trachtenberg, *Reparations in World Politics*, 85–86, 97, 135.

ments of European stability based on the superior power of an alliance of Western powers over a weaker and restrained Germany—containment—contradicted the needs of a stable world order legitimized by the principle of an international system of self-determining, democratic, and capitalist nation states that included Germany—reintegration. Second, the requisites of a European balance of power conflicted with those of economic recovery. Recovery required that Germany achieve and maintain a prosperous industrial economy, both to repay reparations and to stimulate European trade, but the balance of national capabilities demanded that Germany be denied the political and military hegemony inherent in its industrial and human resources. Third, there was a contradiction between the formation and execution of the peace settlement. The Versailles Treaty imposed the victors' conditions on the vanquished and took German hostility for granted; it therefore incorporated few incentives for fulfillment. Yet, the reparation and disarmament clauses in particular required German cooperation, good will, and good faith in compliance. And, fourth, the requirements of internal and international stability were contradictory. The successive coalitions that governed the Weimar Republic sought the continued, incremental revision of the Versailles Treaty, but those governing the Third Republic constantly sought German adherence to its original provisions. Both the French and German positions were based on the need to maintain internal political consensus, but the achievements in foreign policy that the two nations sought were mutually antagonistic. For the two countries to have agreed to a compromise international settlement, one that included both reparations and war debts, the leaders in each would have had to confront the socially divisive decisions on how the costs of the war would be apportioned at home.

Works written from all three perspectives—whether they emphasize responsibility, conflicting interests, or systemic contradictions—often share a common sentiment of ambivalence and irresolution, one that is at times stated explicitly but that most often remains only implicit. The belief that the Second World War was unnecessary is combined with the suspicion that the efforts of the 1920s toward peace and stability were condemned to failure in advance. As a result, the fundamental question about the 1920s continues to be asked in many forms: Did the terms on which World War I was concluded require another war? Could the peace settlement of 1919 have been enforced, fulfilled, or revised in a manner such as to promote durable, peaceful stability? Was “a broken world,” to use Raymond J. Sontag’s term, broken beyond repair?⁹

The most pessimistic answer to these sorts of questions is one of the most popular. The 1920s were an anomaly and an interlude, a pause on the road to depression and dictatorship and a brief truce in the century of total war. The decade of peace was an era of illusions.¹⁰ Of all of the interpretations of “the twenties problem,” this one attributes to the time between the end of the First World War and the rise of Hitler the least historical significance. Recent scholarship, which suggests that the 1920s are more crucial, issues two challenges to this

⁹ Sontag, *A Broken World, 1919–1939* (New York, 1971), vx.

¹⁰ For the most recent restatement of this position, see Marks, *The Illusion of Peace*.

view. The first sees the 1920s not as an interlude but as a continuation of the hostilities of World War I by diplomatic instead of military means, as a struggle that was not resolved and that eventually led to the outbreak of World War II. Thus, both “the era of illusions thesis” and “the continuity of hostilities thesis” tend to discount the significance of the international stability that was achieved following World War I.

By contrast, the second challenge emphasizes the importance of the efforts at stabilization made during the decade, efforts that were unsuccessful from 1919 through the Ruhr invasion of 1923 but that resulted in tentative stabilization beginning in 1924.¹¹ This stabilization is viewed as part of a longer process that then culminated during the years from the Marshall Plan to the formation of the European Economic Community. From this perspective, the 1920s are seen as the decade during which the new models of political organization, which triumphed after World War II, developed in rudimentary form. These include the corporatist pluralism that Maier has analyzed for industrial society, the supranational integration that McDougall examined for the European state system, and the functioning of the Atlantic community that Artaud discussed for the world political economy. All three institutional structures, McDougall has argued, manifested a shift in Western political values away from a belief in nonintervention in a self-regulating society toward *étatisme*, *dirigisme*, and “international economic management.”¹²

In this context, the two postwar eras have come to be viewed within a single time frame. The argument in support of such an interpretation was initially propounded by Arno J. Mayer and continued by N. Gordon Levin, Jr. The Mayer thesis sees the international politics of the first half of the twentieth century as an internal and international civil war between American-led democratic capitalism and Russian-led revolutionary socialism, with Germany as the reactive but not passive object in the struggle for control of Europe.¹³ One strength of this view lies in the manner in which the theory and practice of the domestic and foreign policies of Europe and the United States are integrated into a single, coherent analysis. More recently, Charles Maier has put forth a second integrated analysis of twentieth-century international politics. His approach is that of political economy rather than of ideology, and the events of the 1920s play a more pivotal role in his thesis than they do in Mayer's. The major conflict of the first half of the twentieth century centered on the efforts of the United States to defeat the threat posed by German projects for autarky first under Ludendorff and then under Hitler to the American vision of a liberal, pluralist, political economy for Europe and for the Atlantic region and then to incorporate German economic capacity into that order. The integration of Germany in turn required America to “assume the burden of funding Germany's international deficits—including reparations—after the two world wars.” The

¹¹ See below, pages 644–45.

¹² Maier, *Recasting Bourgeois Europe*; McDougall, “Political Economy versus National Sovereignty,” and Review of Rupieper's *The Cuno Government and Reparations, 1922–1923*, 363–64; and Denise Artaud, *La Reconstruction de l'Europe, 1919–1929* (Paris, 1973).

¹³ Mayer, *Political Origins of the New Diplomacy, 1917–1918* (New Haven, 1959), and *Politics and Diplomacy of Peacemaking: Containment and Counterrevolution at Versailles, 1918–1919* (New York, 1967); and Levin, *Woodrow Wilson and World Politics: America's Response to War and Revolution* (New York, 1968).

reluctance with which the United States did so after World War I delayed international stabilization until after the Dawes Plan of 1924. "U.S. willingness to take on the burdens after 1947 facilitated the stabilization of the 1950s and 1960s."¹⁴

Thus, the passage of time and subsequent historical developments have led to an interpretation of the 1920s that sees the decade as more than a time of illusion, more than a prelude to the 1930s, and more than an element in a teleology of 1939. The third decade of the twentieth century was, instead, the time when the conditions for international stability were defined. The limitations of a return to the pre-World War I international system were reached, and the plans were drawn and the first efforts made toward what became the post-World War II settlement. This interpretation is a significant historiographical departure.

THE OBJECTIVES AND STRATEGIES OF FRENCH FOREIGN POLICY provide the focus for the works of Schuker, McDougall, Bari  ty, and Trachtenberg, as well as, less exclusively, those of Maier, Artaud, and Silverman. The conclusions they put forth amount to an important revision. These scholars have rejected the conventional interpretation that, stated in its extreme form, assigns to French foreign policy from Versailles to the Ruhr a relentless, coherent, and coordinated effort, driven by vengeance, to maintain a permanently isolated and defeated Germany. Because, in addition, Paris demanded reparation far beyond German debtor capacity to pay and Allied creditor capacity to receive, French policy was the major barrier to the re-establishment of peaceful stability in Europe.

By contrast, the works under discussion here contend that French policy offered the best prospect for a stable, peaceful Europe and aimed at accommodation with Germany. According to Stephen Schuker and Marc Trachtenberg, French reparation demands were moderate from the Paris Peace Conference to the Dawes Plan. France was insolvent, lacking the resources to maintain its international position—whether that be defined, in Schuker's view, as predominance in Europe or, in McDougall's terms, as survival with other "Great Powers" within a European equilibrium. Not only was France militarily dependent on other states for security in the event of German rearmament but, as Schuker and Bari  ty have pointed out, it was also financially dependent on foreign capital and industrially dependent on foreign sources of raw materials and energy. This threefold lack of self-sufficiency, McDougall concluded, left France in an inherently weak position within the international system. French foreign policy was, moreover, critically incoherent. Conflicting objectives were pursued with confused strategies, both because of the fundamental dilemmas, based on inherent internal contradictions, that confronted French policy makers and because of French institutional and personal conflicts. The army did not speak with one voice on security policy, and French industry

¹⁴ Maier, "The Two Postwar Eras and the Conditions for Stability," 348–51, and *Recasting Bourgeois Europe*, 579. These issues were further explored at "The German-American Symposium on the United States and European Recovery after Two World Wars," held in Berkeley, July 1982, a conference organized by Erich Angermann and Gerald D. Feldman.

never reached a consensus on economic foreign policy.¹⁵ And, as Schuker has noted, permanent bureaucracies in key ministries—foreign affairs, finance, and commerce—offered conflicting advice on crucial issues. Postwar French governments did not coordinate policy or planning with either corporatist interest groups or the military elite, nor were they able, as Maier and Schuker have stressed, to overcome bureaucratic rivalries.

Indeed, from the Peace Conference of 1919 through the liquidation of the Ruhr invasion in 1923–24, two policy complexes prevailed in Paris. One aimed at achieving a permanent, internationally guaranteed reparations settlement, obtained through the integral implementation of the Treaty of Versailles, a policy that required the stabilization and recovery of Germany. The other involved the destabilization and disaggregation of Germany, worked for substantive revision of the Versailles settlement, and risked the loss of Anglo-American support. Under this policy, French security was to be guaranteed by a special status for the Rhineland, reparation was to be paid—if necessary—through direct exploitation of the occupied territories, and economic strength and security were to be achieved by the merger of French and German heavy industry under French hegemony.¹⁶ The first strand was reactive, moderate, and legalistic; the second was aggressive, ambitious, and revisionist. The two worked at cross purposes at crucial junctures, and the contradictions were present even in the policies of Raymond Poincaré, French premier from 1922 to 1924, who has usually been regarded as the most purposeful of French politicians, and in the Ruhr invasion, conventionally regarded as the most decisive of French actions.

Constraints were imposed, as both Maier and Schuker have pointed out, first on Poincaré and then on Edouard Herriot, French premier in 1924 and 1925, by the sagging French economy and by the configurations of French party politics. Each leader was restrained by personal-political values, and each was hindered by deficiencies of statesmanship. All this left the French unable to sustain either a strategy of independent treaty enforcement or one of independent revision, so that France was highly vulnerable to Anglo-American mediation. Bariéty and Trachtenberg have noted Poincaré's hesitancy, uncertainty, and inability to exploit to French advantage his victory over German passive resistance in the Ruhr in October–December 1923, and Schuker has pointed to Herriot's ingenuousness, ineptitude, and indecision at the London Conference of July–August 1924 and his inability to weld French policy into an effective strategy and a coherent set of demands in exchange for the end of French military occupation of the Ruhr. As a result, the Ruhr invasion was liquidated and the Dawes Plan accepted on terms so unfavorable as to represent the definitive interwar defeat of French foreign policy.

In these terms, there is a new history of French foreign policy, a history based on significant new data, deeper understanding, and revised interpretations. There is

¹⁵ On the diversity and uncertainties of French foreign economic policy, see the works cited in note 27, below.

¹⁶ While Schuker has insisted that French policy was strictly "implementationist," McDougall has emphasized the "revisionist" strand; Schuker, *The End of French Predominance in Europe*, esp. 171–72, 214, 336–37; and McDougall, "Political Economy versus National Sovereignty," 4–23.

in such a revision both much to commend and much to criticize.¹⁷ It is, at most, only half the story of the postwar years—the more deterministic half. The remainder is concerned with the potential for a different outcome, with the possibilities for avoiding the defeat of 1924 and for constructing a different international order in which France's Versailles advantages over Germany were retained and France's position as a great power was preserved. The new international history of the 1920s, therefore, comprises a dual effort. French policy and the results of France's relations with the other powers have been researched and documented, and the unattained potentialities of France's position in world politics after 1919 and the unrealized alternatives open to French policy makers have been explored. Both are significant contributions to an understanding of the decade.

Unfortunately, the position of France among the powers of Europe after World War I has not been as clearly delineated. On the one hand, Schuker has suggested that France held a position of predominance in Europe that ended in 1924, but, on the other, he has not substantiated French secular primacy. His work, on the contrary, illustrates French weakness and vulnerability—the loss of the transitory diplomatic ascendancy France achieved as the result of the advantages conferred by the Versailles Treaty. By contrast, Bariéty avoided the concept of primacy and posited instead Franco-German parity, seeing both states as “very great powers” in 1919. McDougall has regarded French diplomacy as an attempt not to maintain predominance but instead to survive as a great power within a European equilibrium and to achieve first “long-term parity” and “then partnership with Germany,” Sally Marks has seen French foreign policy as an effort to defend France's victory in the war, and Artaud has suggested that France had lost its position as a great power by 1922 and sought to regain in the Ruhr.¹⁸ A rather confused picture has resulted. “French predominance” and like terminology, however, actually obscure rather than clarify both the power relations of postwar Europe and the intentions of French policy makers.¹⁹

An analysis of the position of France in postwar Europe requires a clear determination of precisely which elements of power were foundational and which superstructural, which were intrinsically durable and which circumstantial, which were actual and which potential. Immediately following World War I, French power resided chiefly in its military strength; not only was the French army the best manned and best equipped in the world, but it occupied an advanced geographical position inside German territory. French military strength and French military presence afforded security to Western Europe, deterred German territorial ambi-

¹⁷ For an analysis of the recent revisionist scholarship, see Jon Jacobson, “The Strategies of French Foreign Policy after World War I,” *Journal of Modern History*, 55 (1983): 78–95.

¹⁸ Bariéty, *Relations franco-allemandes*, 753; McDougall, *France's Rhineland Diplomacy, 1914–1924*, 16, 36, 369; Sally Marks, “The Myths of Reparations,” *Central European History*, 11 (1978): 244–45; and Denise Artaud, “Die Hintergründe der Ruhrbesetzung, 1923: Das Problem der interalliierten Schulden,” *Vierteljahrshefte für Zeitgeschichte*, 27 (1979): 241–42.

¹⁹ “French predominance” has been repeatedly criticized on several grounds; see, for example, Sally Marks, Review of Schuker's *The End of French Predominance in Europe*, in *Central European History*, 10 (1977): 75; Silverman, *Reconstructing Europe*, 233–37; Trachtenberg, *Reparation in World Politics*, ix; and Jacobson, “French Foreign Policy after World War I,” 80, 82.

tions in Eastern Europe, and limited the options of German foreign policy. France provided, moreover, the connecting link in the coalition that had stopped the German military machine in 1918 and potentially benefited from the authority, the restraints, and the sanctions of the Versailles Treaty. Such a military and diplomatic position gave Paris a more influential place in European politics than the substance of French power permitted. For France had been weakened by the devastation of war, was lagging in industrial development, was dependent on the Americans and the British for security in the event of German military recovery, and was vulnerable to the political pressures of foreign capital. Granted, German military and diplomatic capability was restricted by defeat, occupation, demilitarization, disarmament, and isolation. But many intrinsic German advantages—the demographic and economic prerequisites of European hegemony and world power—survived the war and the peace: population, raw materials and energy resources, and industrial organization and facilities. Political potential survived as well. Provided that Berlin did not rival or challenge the world positions of Britain and the United States, it could, by exploiting Germany's geographical position, establish relations with the Soviet Union just close enough to help worry Washington and London into a policy of conciliation.

If the balance of power in Europe was not to tip irrevocably in Germany's favor, French policy makers had to ensure that restraints on German military capabilities and political options were maintained both by enforcing the treaty structure of 1919 and by preserving Franco-Anglo-American solidarity, a task that was often contradictory and about which much has been written. They also had to try to exploit any additional opportunities to increase French power, a task that also often involved contradictions and about which much recent scholarship has been concerned. France might have developed, over the long term, the industrial infrastructure necessary to support the preponderant political position temporarily ensured by the Treaty of Versailles and might have used reparation payments to transfer real wealth from Germany and to expropriate German capital and German resources. Under such circumstances, Germany might have been permanently weakened by limitations on its national sovereignty and restrictions on its economic power, preparing the way for Franco-German economic integration. Had France been able to capitalize on opportunities of this magnitude, the balance of power in Europe would have been decisively altered, even after the Paris Peace Conference, through the displacement of industrial capacity and military potential from Germany to France. The implication of this hypothesis is that the immediate postwar period was a much more crucial time for the establishment of a new European balance of power than scholars have previously recognized—as crucial as the destruction of the military status quo on the Continent from the remilitarization of the Rhineland in 1936 to the invasion of Russia in 1941.

For Maier, the 1920s are the fulcrum of the twentieth century. He has argued that the European stability that emerged during this decade was achieved on terms that were fundamentally different from those on which the stability of pre-World War I Europe was based and similar to the terms of the post-World War II stabilization of the 1950s and 1960s. The fundamental contest of the 1920s was a

struggle to determine the dominant mode of political economy for the twentieth century. Maier has supported this interpretation by analyzing the emergence of corporate pluralism and the demise of the liberal, noninterventionist state in France, Italy, and Germany. Although much of Maier's analysis is couched in terms of comparative national history, he has suggested that important issues for the international system were at stake in Europe's shift to corporatism.

Maier has posited three propositions for international history that have received general acceptance: (1) Neither the armistice nor the peace treaty brought stability to Europe. Late 1918 to early 1924 was a period of delayed stabilization in international relations as well as in internal political economy. After various false starts and failed alternatives, Europe achieved a tenuous stability in the years 1924–26 that lasted until the Depression. (2) Who won and who lost in the 1920s was reflected in how the costs of the war were distributed between the victors and the Germans through reparations, among the Allies themselves through war debt payments, and among various interests and classes through inflation. And (3) the relative national power of France and Germany within the European state system was dependent on the relative strength of public and private power within the political economies of Western Europe during the years of struggle for stabilization, particularly during the Ruhr crisis of 1923. Here, the sovereignty of the Berlin government and the unity of the Reich were in serious jeopardy. Both, according to Maier, dissolved under the dual pressures of foreign occupation and hyperinflation. So much of the power of the German state was distributed among antagonistic special interests, regional interests, and foreign interests that Germany ceased to function coherently in the European state system.²⁰ Although a German national interest was eventually defended against private interests, and although national unity did survive particularism and secessionism, the threat to German unity, sovereignty, and power was very real during these years. Maier's analysis thus poses a challenge to the prevailing interpretation put forth by A. J. P. Taylor and Gerhard Weinberg that these issues were resolved once and for all by the terms of the armistice and the Versailles Treaty.²¹

According to Bariéty, the French harbored a plan to exploit what he has termed their "momentary surplus of power in international life." The plan was to undermine Germany's prewar economic hegemony on the Continent, to achieve energy independence, to make France into Europe's leading iron and steel producer, and so to develop the industrial basis to support and maintain over the long term the position of politicomilitary superiority that the settlement of 1919 had temporarily handed France. "Un projet sidérurgique français" was embedded, he has maintained, in the territorial, commercial, and reparations clauses of the Treaty of Versailles, which returned to France the iron deposits of Lorraine and gave to France control over the coal of the Saar and not only capital transfers but also additional coal and coke from reparations payments. Bariéty has called Versailles "a declaration of war on German industry," and the Ruhr invasion

²⁰ Maier, *Recasting Bourgeois Europe*, esp. chap. 6.

²¹ Taylor, *The Origins of the Second World War* (New York, 1962), chap. 2; and Weinberg, "The Defeat of Germany in 1918 and the European Balance of Power," *Central European History*, 2 (1969): 248–60.

represented France's "supreme effort" to achieve these aims. Thus, for Bariéty, the central issue of international relations during the immediate post-World War I years was a Franco-German contest for industrial supremacy and for control of the Rhineland and the Ruhr, the keys to victory in that struggle.²²

If Maier has shown that German political organization remained fragile at least until 1924 and Bariéty has demonstrated the potential for French economic resurgence after 1918, McDougall has hypothesized that a weakened Germany and a strengthened France could have been integrated into a West European community on terms of parity and partnership. In his view, the fundamental contest of the decade was a struggle between two alternative models for international stabilization in the twentieth century and the ways to remove what was the main barrier to that stability—the problem of German power. One of these means is familiar: a system of sovereign and competing nation-states and the punishment and probation of the offender with the ultimate restitution of a democratized Germany to a place within the system. He has characterized this approach as Wilsonian democratic nationalism, an approach that was discredited by its failure during the years 1933–45. The other model is that of West European unity based on the economic integration of France and Germany, supported by the British and the Americans. The necessary prerequisite to this integration was the imposition of limitations on the sovereignty of the Reich over the economic and military potential of the Rhineland and the Ruhr, the permanent weakening of German power, and the establishment of Franco-German parity. McDougall has termed this alternative "structural integration," an approach that was vindicated by the long period of peaceful stability since World War II under the European Coal and Steel Community and the Common Market. Because it has proved to be the surest road to European stability, it was a more progressive policy for the 1920s than the Anglo-American alternative of defending German unity and sovereignty. Indeed, this approach was, in McDougall's view, the only realistic solution. Germany could have been safely reintegrated into Europe and a lasting equilibrium established on the Continent only if German power were diminished.²³

The hypothetical potential for German disaggregation, French reindustrialization, and European integration has received critical consideration and evoked serious objections. Two questions are most fundamental: Were such opportunities inherent in the actual historical conditions of international politics during the 1920s? And was it the conscious and consistent intention of French policy makers to exploit them? Recent research points to an affirmative answer to the first of these questions. The unity of the Reich and the sovereignty of the Berlin government, as Maier has noted, were tenuous during the revolution of 1918–19 and again during the Ruhr crisis. The peace settlement itself, according to McDougall, gave recognition to the principle of "overlapping sovereignties" in the Saar, in Danzig, and

²² Bariéty, *Relations franco-allemandes*, 126–49, 750, 753–55, and "Le Rôle de la minette dans la sidérurgie allemande et la restructuration de la sidérurgie allemande après le traité de Versailles," Centre de recherches des relations internationales de l'Université de Metz, *Travaux et recherches* (1973), 233–77.

²³ McDougall, "Political Economy versus National Sovereignty," 4–7, 9, 11, and *France's Rhineland Diplomacy, 1914–1924*, 13, 82, 29–31, 168, 364–65.

elsewhere and thus established a principle that could be extended to the Rhineland and the Ruhr. And, as Bariéty has pointed out, contained in the Versailles Treaty was the basis for integrating not only the French and German iron and steel industries but also their chemical industries and canal systems.

The answer to the second question is more complex. The continuation of interallied economic cooperation and of Anglo-American economic support for France into the peacetime era was, for Trachtenberg, the central feature of French wartime visions of a "new economic order." Soutou has noted that projects for bilateral economic cooperation with Germany, industrial interpenetration, and commercial agreement, arrived at through direct bargaining between Paris and Berlin, were inherent in French attempts at workable reparation payment schemes, schemes that are epitomized by the Seydoux Plan of 1920–21. The restriction of German power through the limitation of German sovereignty and the abridgment of German unity was, Soutou, McDougall, and others have claimed, a feature of various French projects to give the Rhineland a special status, beginning with the outbreak of the First World War and culminating in Poincaré's efforts after the failure of German passive resistance in the Ruhr.²⁴

Yet questions regarding the feasibility of French visions and projects persist.²⁵ Would German policy makers, with their state and economy weakened through the loss of control over the Ruhr and the Rhineland, have been willing—or even able—to pay cash reparations, buy French products, and integrate with France economically? Was not the political disaggregation of Germany incompatible with genuine reconciliation with Germany rather than the preliminary to it? Was detente through intimidation possible given the heated nationalism of the postwar period? Would fulfillment of the London Schedule of 1921, a reparations plan that some scholars have depicted as relatively moderate, have sufficed to transfer wealth on a scale necessary to redress the balance of economic power? Was the task of reindustrializing France a simple matter of expropriating German resources and capital?

Post–World War II stability has depended on four institutional complexes: the division of Germany, Franco–West German reconciliation, West European supranational integration, and the Atlantic community. Historically, the advocates of these complexes were not so strong following the settlement of 1918–19 as they

²⁴ Trachtenberg, *Reparation in World Politics*, chap. 1; Soutou, "Die deutschen Reparationen und das Seydoux-Projekt, 1920–21," *Vierteljahrshefte für Zeitgeschichte*, 23 (1975): 237–70, and "La France et les marches de l'Est, 1914–1919," *Revue historique*, 528 (1978): 341–88; D. Stevenson, *French War Aims against Germany, 1914–1919* (Oxford, 1982); and McDougall, *France's Rhineland Diplomacy, 1914–1924*. For an analysis of the scholarly controversy over French policy during the Ruhr crisis, see Jacobson, "Strategies of French Foreign Policy after World War I," 87–92.

²⁵ Responses to McDougall's thesis can be found in Charles S. Maier, "The 'Truth about Treaties?'" *Journal of Modern History*, 51 (1979): 57–67; Schwabe, Comment on McDougall's "Political Economy versus National Sovereignty," 70–73; and Krüger, "Das Reparationsproblem der Weimarer Republik," 44–47. Krüger has cast doubt on whether the weakening of German economic potential was desirable from the perspective of the prosperity and stability of the international political economy, arguing that Germany could have been more productively and more securely integrated with the West through international economic cooperation, including a redistribution of war costs within a liberal world economy. Krüger's position depends heavily on the presumed willingness both of the United States to maintain the long-term burdens of world economic leadership and of Germany to subject itself to the pacifying influence of indirect U.S. control.

became following the settlement of 1945; the conditions for any one of them were not so favorable in the 1920s as they were in the 1950s; and the four elements were not so compatible in the twenty years before the Second World War as they were in the two decades after it. The potential for integrationist stability was, therefore, not nearly so great. Franco-German accommodation after World War II resulted from circumstances not present after World War I. The decisive defeat of France in 1940 and of Germany in 1945, along with the threats of Russian domination and of an American protectorate, made allies of long-time enemies. Hitler discredited the nondemocratic Right and Stalin the nondemocratic Left, leaving a democratic center eligible for and receptive to American support. And the United States after World War II made a direct and continuous commitment not only to the reconstruction of Europe's economy but also to the preservation of political stability and military equilibrium. Therefore, German economic potential could develop fully after the Second World War without creating a military threat to the independence of the other nations of Western Europe.

The economic recovery of Germany in turn buttressed political stability and peace, weakening radical alternatives within Germany and dissipating drives for *revanche*. In the 1920s, by contrast, British and U.S. support for France (at least as it was envisioned in Paris to encompass military protection, war-debt forgiveness, and shared raw materials and energy resources) was not always forthcoming; therefore, accommodation with Germany emerged in French policy at times when Paris was most frustrated with London and Washington and as an alternative to dependence on the British and the Americans, not as a complement to Allied solidarity. Nor could the weakening of Germany and reconciliation with the West constitute part of the same stabilization package under post-1918 conditions. The disaggregation of Germany and direct French exploitation of the economy of the Rhineland and the Ruhr would have prepared the way not for Franco-German rapprochement but for German isolation, for prolonged nationalist resentment, and for international instability. It was not the alienation of the Rhineland and the Ruhr from Germany but the security afforded to this region by the Locarno Treaties of 1925 that promoted both the popular and business support for whatever Franco-German accommodation and West European economic integration that were achieved between the wars.²⁶ Thus, the model of "structural integration" that McDougall has posited demonstrates that the two postwar eras can be viewed within a single time frame but that the elements of stability that were more comprehensive, conclusive, and durable after 1945 were still rudimentary, fragmentary, and ephemeral after 1918 and that the resistance to integration—that is, competitive national sovereignties—remained strong and ultimately prevailed in the 1920s.

On what terms did French policy makers and industrialists intend to cooperate with Germany economically and to arrive at a political detente? Recent research has discovered a diversity of uncoordinated schemes extending from the time of the

²⁶ Bariéty has shown that economic collaboration followed from, rather than preceded, political accommodation; see his "Industriels allemandes et industriels français à l'époque de la république de Weimar," *Revue d'Allemagne*, 4 (1974): 1–16.

Paris Peace Conference to the International Steel Cartel of 1926.²⁷ Further analysis, evaluation, and synthesis are necessary before we can finally determine which French projects were aimed at reconciliation with Germany on the basis of parity and partnership, which were attempts to maintain French political predominance in exchange for economic equality, and which were endeavors to force Germany into economic inferiority as well. Debate may well take place between two interpretations. One of these, set forth by McDougall, Trachtenberg, and Hermann Rupieper, emphasizes the projects of French advisers, particularly those proposed in 1922–23 before and during the Ruhr invasion—projects to use France’s military superiority over Germany, to exploit the Rhineland and the Ruhr directly, to take advantage of German economic collapse and political disintegration, to provide territorial guarantees of French security through a radical alteration of the status of the Rhineland, to acquire permanently the coal mines of the Ruhr and the Saar, and to assure a market for French steel in Germany through favorable tariff agreements. The other viewpoint emphasizes Poincaré’s lack of interest in any feature of such projects and his pursuit instead of an international reparation settlement in fulfillment of French treaty rights, a position maintained by Schuker, and it stresses the basic lack of commitment by both French and German industrialists to merging the French and German iron and steel industries, an argument made most fully by Bariéty.²⁸

YET THERE WERE PROSPECTS FOR RESTRUCTURING RELATIONS among the Western Allies on bases other than national competition. What was chiefly at stake in international relations during the 1920s—according to Artaud, Trachtenberg, and Silverman—was the endurance of wartime Allied solidarity into peacetime in the form of economic cooperation as well as military alliance. At issue were both how the costs of the war would be distributed among the victors and how the capital, raw materials, and energy resources—the basis of modern state power—would be allocated among them.

Trachtenberg has maintained that the opportunity for an international order based on Allied solidarity was present in the plans for postwar reconstruction

²⁷ Jacques Bariéty, “Le Rôle d’Émile Mayrisch entre les sidérurgies allemande et française après la première guerre mondiale,” *Relations internationales*, no. 1 (1974): 123–34, “Le Rôle de la minette dans la sidérurgie allemande,” 233–77, “Industriels allemands et industriels français,” 1–16, *Relations franco-allemandes*, chaps. 3–4, and “Das Zustandekommen der Internationalen Rohstahlgemeinschaft, 1926: Alternative zum misslungenen ‘Schwerindustriellen Projekt’ des Versailler Vertrages,” in Hans Mommsen et al., eds., *Industrielles System und politische Entwicklung in der Weimarer Republik* (Düsseldorf, 1974), 552–68; and Georges Soutou, “Problèmes concernant le rétablissement des relations économiques franco-allemandes,” *Francia*, 2 (1974): 580–96, “Der Einfluss der Schwerindustrie auf die Gestaltung der Frankreichpolitik Deutschlands, 1919–1921,” in Mommsen et al., *Industrielles System und politische Entwicklung*, 543–63, “Die deutschen Reparationen und das Seydoux-Projekt, 1920–21,” 237–70, and “Les Mines de Silésie et la rivalité franco-allemande, 1920–23: Arme économique ou bonne affaire?” *Relations internationales*, no. 1 (1974): 135–54. Also see Peter Krüger, *Deutschland und die Reparationen, 1918/19* (Stuttgart, 1973), 131–37; Maier, *Recasting Bourgeois Europe*, 194–209, 516–45; and Schuker, *The End of French Predominance in Europe*, 219–29, 332–34, 359–73.

²⁸ McDougall, *France’s Rhineland Diplomacy, 1914–1924*, chaps. 6–8, and “Political Economy versus National Sovereignty,” 16–21; Trachtenberg, *Reparation in World Politics*, 265–75, 305–08; Rupieper, *The Cuno Government and Reparations, 1922–1923*, 124–29, 257–58; Schuker, *The End of French Predominance in Europe*, 171–72, 214, 336–37; and Bariéty, “Industriels allemands et industriels français,” 1–16.

developed by Étienne Clementel, the French minister of commerce from 1915 through 1919, who hoped to reorganize both world trade and French industry into a “new economic order.” The free market would be suppressed; a complete system of economic planning would be introduced at both the national and the international levels; and the postwar economy would be ordered by cooperation rather than by competition. During the war and armistice period, Trachtenberg argued, the French program for reconstruction had as its primary goal not reparations from Germany but the conversion of wartime Allied cooperation into a permanent Allied economic union to reallocate primary products—industrial raw materials, energy resources, and agricultural commodities—at fixed prices and according to need. Although this project never came to fruition, the maintenance of Allied solidarity remained the central principle of French foreign policy at the Paris Peace Conference, where it both determined the security policy of Georges Clemenceau and influenced French reparations policy.

The enormous demand for reparations put forth initially by the French delegation, Trachtenberg claimed, was not intended as a serious goal but as a strategy to coerce the Germans into concessions, particularly the delivery of needed raw materials, and as a bargaining chip to be conceded to the United States in exchange for financial assistance. What costs the German economy could not sustain, the Allies were to agree to share in a “community of sacrifice” or “a financial League of Nations”—a world fund for the reconstruction of the devastated areas: war costs would be pooled and interallied debts would be reapportioned, with each nation paying according to its ability.²⁹ After the initial stage of the peace conference, however, Clementel’s schemes ceased to dominate French reparation policy. By late March 1919, Paris had aligned itself with London in favor of including separation allowances and pensions in the reparations bill and of postponing setting a fixed sum until May 1921. At the same time, French reparation policy became closely linked with the ambitions of French Rhineland policy within Clemenceau’s complex peace strategy. Once initial French demands for a Rhenish buffer state had been denied, an open-ended financial settlement came to be seen as a means by which the military occupation of the Rhineland—the means by which the treaty was enforced—could be continued indefinitely, maintaining a French presence on the Rhine without violating the treaty or directly challenging the Allies.³⁰ Above all, Clementel’s *idée fixe* never had any international standing. Woodrow Wilson and his advisers repeatedly refused even to consider the continuation of wartime controls over the allocation of primary products and the redistribution of war costs, and British policy on this issue closely paralleled U.S. thinking. For the wartime financial entente with France, Silverman has argued, London and Washington substituted an Anglo-American special relationship that subordinated the economic recovery of France to the vision of revitalized trade with Russia, as David Lloyd George wished, and to the financial reconstruction of Germany, as both he and Wilson’s Republican successors desired.³¹

²⁹ Trachtenberg, *Reparation in World Politics*, chaps. 1–2.

³⁰ Stevenson, *French War Aims against Germany, 1914–1919*, chap. 7.

³¹ Silverman, *Reconstructing Europe after the Great War*, 20–27, 32–38, 46–52, 229–70.

The breakdown of Allied economic cooperation was significant because the potential benefits of a postwar stabilization based on continued Allied solidarity would have been—as Artaud and Trachtenberg have suggested—far-reaching. A redistribution of war costs in 1919 might not only have removed a source of division among the Allies but also have made possible an early compromise reparations settlement, eliminating a major obstacle to Franco-German reconciliation and precluding the Ruhr invasion. Monetary stabilization might have been accomplished earlier and prevented hyperinflation in Germany in 1923 and the fall of the franc in 1924–26. The sharing of raw materials, a prompt settlement of war debts and reparations, and immediate American loans to Europe might have facilitated the rapid economic reconstruction of the Continent. A recovering German economy might have been reintegrated into a system of Allied economic unity without dominating it; failing that, a permanent Allied economic bloc might have been strong enough to counterbalance German power. In either case, the German threat to the peace and stability of Europe would have been substantially lessened.³²

If postwar stabilization based on continued Allied economic solidarity was potentially so beneficial, why did not American policy makers recognize the principle of “an equitable sharing of war costs” and cancel or reduce war debts to a level more readily acceptable to the debtors? Artaud looked beyond the conventional answer, which holds that the opposition of Congress and public opinion constrained a more willing executive branch, and maintained that the United States in 1919 lacked the world financial supremacy that subsequently allowed it to cancel war debts and to fund international deficits after World War II and that cancellation was not in the national interest of an America engaged in rivalry with Great Britain for just such financial leadership after 1919. The British cabinet in early 1920, Silverman has demonstrated, considered unilaterally canceling the war debts owed England in an attempt to shame the United States into doing likewise. But instead London first appealed to Washington for a general cancellation, and, when this failed, the government announced its intention—in the Balfour Note of August 1922—to collect from its own debtors and consolidated its war debt with Washington in January 1923. Paris was then confronted with an Anglo-American united front, demanding of France war debt payment without guaranteed reparation income from Germany. Artaud has argued that France’s resulting isolation from its former allies contributed to the decision to invade the Ruhr.³³

The actual basis for the financial-economic stabilization of the second half of the 1920s—a basis that was laid in the years 1922–26—was, then, war debt consolidation, reparations from Germany, and U.S. loans. These financial links, as well as American arbitration of intra-European disputes and a common anti-Communist ideology, were the elements of what Artaud has termed “a nascent Atlantic Community.” The dollar gap, which was created by the collection of war debts and the concurrent sale of primary products and manufactured goods abroad and the

³² Artaud, *La Question des dettes interalliées*, 926; and Trachtenberg, *Reparation in World Politics*, 2–4, 15–16.

³³ Denise Artaud, *La Question des dettes interalliées*, i–xi, 924–36, “La Question des dettes interalliées et la reconstruction de l’Europe,” *Revue historique*, 530 (1979): 363–73, 376–77, 379–82, and “Die Hintergründe der Ruhrbesetzung, 1923,” 357–59; and Silverman, *Reconstructing Europe after the Great War*, 147–61, 186–93.

maintenance of high tariffs, was funded with loans from New York. Washington used American financial power to promote a general settlement of reparations and war debts, with substantial reductions of both, to insist on its political corollary (detente between Berlin and Paris), and to further currency stabilization and the establishment of a market-regulated international gold-exchange standard. Through this multifaceted economic foreign policy, the United States sought to promote a prosperous and peaceful, pluralist and capitalist order in Europe, a Europe that accepted the principle of the open door (equal access to markets and investments) and a Europe modeled on American notions of fiscal rationality (balanced budgets and lowered public expenditure on both land armaments and social programs). Frank C. Costigliola has shown that the American program for the financial reconstruction not only of Europe but of the fragmented world economy was promoted at the expense of the rival British plan for postwar stabilization—regulated capital flows and blocs of countries tied to specific reserve currencies including the pound sterling. Thus, it bolstered U.S. economic power at the expense of Britain and opened the door to American financial predominance.³⁴

Republican foreign policy was neither simply politically isolationist nor economically expansionist. It embraced a persistent effort, Melvin P. Leffler has shown, to reconcile domestic and international economic priorities and to balance an avoidance of strategic commitments abroad with a concern for German rehabilitation and French security. In its most fully developed form, U.S. policy was simple and optimistic in concept: "Believing that a prosperous Germany would be the best safeguard of French security and assuming that a financially sound Europe would be a peaceful Europe, the Republicans focused their major efforts on promoting Germany's economic restoration and expediting Europe's financial stabilization." The executive branch sought to avoid congressional constraints on American intervention and on politically binding commitments to Europe by conducting economic diplomacy through independent experts, private bankers, the Federal Reserve Bank of New York, the agent general for reparation payments, and such theoretically nonpolitical organizations as the World War Debt Funding Commission and the Tariff Commission. After 1922, Republican policy recognized that the stabilization and reconstruction of Europe was in the interest of the United States at a time when American foreign trade and investment were growing and Europe was taking a major portion of each. But Washington did not formally and fully commit American resources either to the redress of German grievances or to the guarantee of French security at a time when the effective disarmament of Germany and the military predominance of France precluded any strategic threat to the United States.³⁵

On the critical issue of Versailles revision, American policy makers pursued a middle course, favoring peaceful change and moderate emendation, promoting neither rigid adherence to the treaty nor its total overthrow. Although the United

³⁴ Costigliola, "Anglo-American Financial Rivalry in the 1920s," *Journal of Economic History*, 37 (1977): 911–34.

³⁵ Leffler, *The Elusive Quest: America's Pursuit of European Stability and French Security, 1919–1933* (Chapel Hill, N.C., 1979).

States encouraged gradual and limited German recovery, German ambitions, Costigliola maintains in a book-length manuscript in preparation, were to be channeled into peaceful economic development.³⁶ With the world economic crisis of 1929–31, however, the United States was no longer able to integrate international and internal imperatives, and Washington found it easier to opt for nationalist solutions at a time when the relative importance of exports to total American industrial and agricultural production remained small and had even declined in some key industries since 1919. The world's largest economy remained relatively autarkical after all. Financially and militarily, Europe was more dependent on the United States than the United States was on Europe. Transatlantic economic and security relations were asymmetrical. Through much of the decade, American intervention was too quasi-official, too lacking in institutional synchronization, and too intermittent and tentative to be inherently stabilizing.

The post-1924 system of stabilization—based on U.S. participation—was partial, contradictory, and fragile. American policy makers, who feared the mutation of the Franco-German detente into an entente that might develop into a coalition of debtors pressing for debt cancellation or into a West European economic bloc more capable of resisting the penetration of U.S. trade and capital, promoted peaceful accommodation and economic cooperation among European nations to the extent that American interests were furthered.³⁷ Meanwhile, British leaders, who expected the whole system of reparations and war debts to collapse in a transfer crisis, paid British war debts after 1923 and accepted the U.S.-sponsored Dawes Plan of 1924 in order to coopt the immense financial power of the United States and especially to secure the support of New York money markets for the British pound, which was pegged at prewar parity in 1926. But the overvalued pound and London's tight money policy produced deflationary pressures and high unemployment without making British prices competitive in the world market. France paid war debts after 1926 to salvage an element of Allied solidarity and to make a "down payment" of sorts on American support as a hedge against German resurgence, but the U.S. doctrine of moderate treaty modification and gradual German recovery became the dominant strategy for European stabilization. Beginning in 1924, Germany borrowed from the United States to give America a stake in German recovery, and German prosperity collapsed with the withdrawal of American credit later in the decade. The exclusion of the Soviet Union from long-term U.S. and European credit confirmed the change in foreign policy from attempts to base national security on peaceful coexistence and partial economic integration with the advanced capitalist states of the West (1921–27) to pursuit of security through autonomous economic development (after 1928).

Monetary stabilization based on the gold standard denied to Europe as a whole the potential benefits of creeping inflation—stimulation of economic growth,

³⁶ Costigliola, "The Dilemmas of 'Gradual Revolution': American Political, Economic, and Cultural Relations with Europe, 1919–1933" (in preparation). The author kindly provided me with a draft manuscript.

³⁷ Werner Link, "Die Beziehungen zwischen der Weimarer Republik und den USA," in Manfred Knapp *et al.*, *Die USA und Deutschland, 1918–1975: Deutsch-amerikanische Beziehungen zwischen Rivalität und Partnerschaft* (Munich, 1978), 77–78.

reduced unemployment, and reduction of the burdens of reparations and war debts in real terms. The flood of American loans to Europe during the second half of the decade went largely uncontrolled; U.S. policy makers and bankers trusted the rationality of the free marketplace, and the loans were increasingly used in an unproductive and speculative manner.³⁸ "This new order," according to Costigliola, "stacked benefits such as trade surpluses, stable prices, and debt receipts heavily in favor of the United States and assigned adjustment burdens such as trade deficits, deflation, and debt payments primarily to Europe and the rest of the world."³⁹ Ever greater infusions of capital became necessary to fund Europe's war costs, interest payments, and trade deficits—increases that continued only so long as European interest rates were higher than New York's. When the collapse came in 1929–31, no nation was willing to pursue multinational solutions at the expense of national interest or able to choose intelligently among alternatives in the absence of suitable economic theory or exact economic intelligence.

THE OLDER HISTORIOGRAPHY of the 1920s praised the British for pursuing the sort of rational, businesslike solutions to the reparations question that would have permitted the happy reconstruction of the European economy; it blamed the French for intransigent, short-sighted, and economically ignorant opposition to British efforts; and it saw the Germans as the unfortunate victims of this conflict. The new history of the 1920s takes for granted German aggressiveness and is skeptical about German willingness to have ultimately accepted the slow and gradual revision of the settlement of 1919; it views French policy as the rational and moderate response to legitimate requirements; it sees U.S. policy as coherent in conception but contradictory in effect; and it blames the British for their inconsistent reparations demands and for their mistaken view that France rather than Germany was the primary threat to European peace and equilibrium. In part, the position taken by the new history is an outgrowth of an asymmetry of analysis, which continues to limit the concept of international history found in studies of the decade. When the formulation of French policy is examined in its full complexity and detail, that policy seems justified. When German and British policy are presented simply in terms of their execution and consequences, they appear less so. The result is the perpetuation of *Nachkriegsschuldfrage* history with a new distribution of postwar guilt among nations, arrived at through the substitution of Germany and England for France as the main barriers to postwar stabilization. Trachtenberg has termed this "Manichaeism in reverse."⁴⁰

³⁸ William C. McNeil has found in U.S. loans to Germany little of the wisdom Schuker attributed to the Morgan partners and little of the direction and control Artaud attributed to the Republican administration; McNeil, "American Money and the German Economy: Economics and Politics on the Eve of the Great Depression" (Ph.D. dissertation, University of California, Berkeley, 1981). Cf. Schuker, *The End of French Predominance in Europe*, 272–83; and Artaud, "Le Gouvernement des États-Unis et le contrôle des emprunts européens, 1921–1929," *Bulletin de la Société d'Histoire Moderne et Contemporaine*, 71 (1972): 17–22.

³⁹ Costigliola, "Anglo-American Financial Rivalry in the 1920s," 915. Also see Maier, *Recasting Bourgeois Europe*, 586–90.

⁴⁰ Marc Trachtenberg, Reply to Comments by Maier, Schwabe, and Wright, 82.

In assigning responsibility to Germany for the obstruction of postwar stabilization, recent scholarship focuses on Berlin's reparation policy. In the fullest recent examination of Germany's capacity to pay, Trachtenberg has concluded that no analysis from modern economic theory can demonstrate that capital transfers of the magnitude discussed at the Paris Peace Conference and set forth in the London Schedule of May 1921 were—or were not—within the limits of Germany's economic capability. The feasibility of reparation could only have been established empirically and politically, through the efforts of Weimar policy. Silverman has agreed, "Capacity to pay, insofar as the term has any meaning at all, is essentially a state of mind. It more accurately reflects the state of the national will than the state of national resources." And Schuker has made the same claim: "The basic problem was German willingness to pay."⁴¹ Reparation payment was, according to this interpretation, merely a matter of political resolve. This is certainly how French policy makers, Poincaré in particular, depicted German policy. But a less partisan analysis needs to inquire into what lay behind what was viewed at the time—in Paris and elsewhere—as German "ill will" and "bad faith."

Recent discussions of the sources of German reparation policy recognize that no band of the German political spectrum to the right of the Social Democrats believed that the Allied reparations invoices, culminating with the London Schedule, were feasible, and they also emphasize the inability of the Weimar governments to bring the representatives of heavy industry to support any coherent and reasonable reparations offer.⁴² An even fuller understanding of the reparations problem can be reached by placing its management in the context of Berlin's efforts to restabilize Germany's finances following the war and then by comparing Germany's problems and efforts with those of the Allies. Silverman has pointed the way to a comparative perspective on postwar financial reconstruction by showing that the payment of war debts was but one of several problems confronting the governments and central banks of Britain and France during the years 1918–23. Currency needed to be stabilized, current budgets balanced, national debts managed, and foreign trade restored. Inherent in these problems were vital issues of national security, fundamental social conflicts over the internal distribution of war costs, and technical complexities of unprecedented magnitude. No generally agreed upon fiscal and monetary theory existed with which to unravel these technical complexities and on which to base economic policy. And in Germany, likewise, payment of reparations was but one of a complex of issues and circumstances involved in the task of financial stabilization.

Berlin had to face that task without the national affirmation of military victory and without the support of the political Right, which attempted to reverse the social achievements of the revolution of 1918–19. Because of the potential magnitude of

⁴¹ Trachtenberg, *Reparation in World Politics*, 66–68, 72–83, 213–16, 219–21; Silverman, *Reconstructing Europe after the Great War*, 146–47; and Schuker, *The End of French Predominance in Europe*, 24. Also see Maier, "The Truth about the Treaties," 62–67.

⁴² For excellent discussions of the dilemmas confronting Berlin over reparation policy, see Ernst Laubach, *Die Politik der Kabinette Wirth, 1921/22* (Lübeck, 1968); Maier, *Recasting Bourgeois Europe*, 243–72, 281–88, 356–73; and Rupieper, *The Cuno Government and Reparations, 1922–23*.

reparations payments, moreover, the politically divisive issue of how the expenses of the war were to be distributed was more acute in Germany than it was in France and England, which had the prospect of reparations income to cover some of the war's costs. Confronted with such difficulties, German policy was understandably confused. As Gerald D. Feldman has demonstrated, German policy makers did not pursue a coherent policy of financial stabilization until forced to do so in 1923 by the internal catastrophe of hyperinflation and the external pressure of the Ruhr occupation. Nor did they pursue a contrived and coordinated policy of deliberately promoting inflation to avoid reparation payment. For the managers of German public finance, neither compliance with the reparations demands of the Allies nor evasion of them was an end in itself. Stabilization policy was subordinated to the need to maintain social peace and to revive industrial production.⁴³ Internal political and economic weakness rather than foreign ambition determined German reparation policy.

This interpretation leads to some rather pessimistic conclusions regarding the implications of reparation payments for the survival of the economic benefits the German working class gained from the revolution and for the viability of the Weimar Republic itself. Feldman has demonstrated that the various unsuccessful attempts to respond to the London Schedule strengthened the political position of the most reactionary representatives of heavy industry and eventually allowed them to determine the terms of German financial stabilization in 1923–24.⁴⁴ Full payment according to the London Schedule over the long term would have required, across a generation or more, a set of conditions that did not exist in 1921–22: a political consensus on social policy, a strong and united left-center parliamentary coalition, access to foreign capital, high industrial productivity, fiscal discipline, high taxation, and moderate inflation. The alternative regime, which might have extracted the surplus necessary to transfer real wealth from Germany to its creditors, was a system of strict controls imposed from within or from without. What was necessary, in other words, was either a dictatorial government with wartime emergency powers over the German economy, as McDougall has argued, or foreign supervision of the German budget or direct exploitation of the economy of the Ruhr, as Trachtenberg has maintained.⁴⁵ The potential cost of the fulfillment of the Versailles Treaty, according to this view, was either the end of democratic conditions in Germany or its loss of economic and territorial sovereignty.

The belief that “the harshness of Versailles” did not cause—and could not have caused—“the collapse of Weimar” has been further challenged by several scholars. The research of William C. McNeil indicates that fulfilling the conditions of the Dawes Plan after 1924 would have had severe deflationary effects on the German economy, had it not been stimulated by foreign loans. Collecting and transferring

⁴³ Feldman, “Reparations and the Domestic Management of German Inflation,” paper delivered at the Lehrman Institute, May 1982.

⁴⁴ Feldman, *Iron and Steel in the German Inflation, 1916–1923* (Princeton, 1977), 427–44.

⁴⁵ McDougall, “Political Economy versus National Sovereignty,” 16, and *France's Rhineland Diplomacy, 1914–1924*, 172–73; and Trachtenberg, *Reparation in World Politics*, 220–30, 238, 240–56, 268–75, 298–301.

the Dawes annuities out of Germany's own resources would have necessitated, theoretically, a deflationary fiscal policy to generate budgetary surpluses and a decline in national income to reduce the purchase of foreign goods and create an export surplus. The resulting social cost was unemployment in the range of 15 to 20 percent, which would have produced an intolerable situation for any political parties willing to undertake genuine fulfillment. In actuality, the uneven flow of capital from the United States and the struggle over its allocation within Germany destabilized both German politics and the economy. David Abraham has argued that the costs of reparation payment, when added to the costs of *Sozialpolitik*, contradicted the necessities of private accumulation in a capitalist economy, which resulted in an offensive by the dominant classes against the republic after 1928. More generally, Peter Krüger has maintained that in an advanced industrial society—one in which the major economic groups are organized to articulate their interests in a politically influential manner, in which there is a general demand for constantly improving standards of living, and in which there is broad popular participation in the political process—the government-enforced deprivation necessary to make reparation payments of the magnitude needed to alter the balance of economic power between France and Germany could have produced only internal political delegitimization and destabilization.⁴⁶ The basic limitation on reparation payments was, according to this interpretation, neither malice nor lack of political resolve or of theoretically estimated economic capacity but the amount of conflict that could be absorbed by the political economy of Weimar Germany.

THE LIQUIDATION OF THE RUHR INVASION, the Dawes Plan of April 1924, and the London Conference of July–August have conventionally been regarded as the first steps toward the most significant detente achieved between Germany and the West during the interwar years. Recently, scholars have examined the terms on which those initial steps were taken and have seen not mutual concession but the threefold and conclusive defeat of French foreign policy. As Schuker has formulated the situation, the French agreed to evacuate the Ruhr, to end their system of productive guarantees, and to relinquish the right to reimpose unilateral sanctions. They did so, moreover, “without any compensation regarding war debts, trade, or, most important, security.” And what they gained in return was inherently unsatisfactory, “a minimum—and obviously impermanent—reparations settlement.”⁴⁷

To complete the interpretation Schuker and others have added several corollaries. (1) The Versailles system collapsed in 1923–24—more than a decade earlier

⁴⁶ McNeil, “American Money and the German Economy,” 162–77; Abraham, *The Collapse of the Weimar Republic: Political Economy and Crisis* (Princeton, 1981), 234–35, 283–84; and Krüger, “Das Reparationsproblem der Weimarer Republik,” 34–40.

⁴⁷ Schuker, *The End of French Predominance in Europe*, 179–80, 186, 343–44, 386–88. McDougall has agreed that the London Conference was “a French surrender” and “a crushing defeat for France,” whereas Bariéty has emphasized French losses short of total defeat; McDougall, *France's Rhineland Diplomacy, 1914–1924*, 367–69; and Bariéty, *Relations franco-allemandes*, 582–84, 791.

than the date conventionally assigned to it. In Schuker's analysis, the London Conference ended the coercion of German compliance with the provisions of the Versailles Treaty through extreme territorial sanctions, either by occupying additional territory (such as the Ruhr) or by prolonging the occupation of the Rhineland indefinitely. This emendation, in turn, undermined the French system of security based on military occupation. In McDougall's formulation, the prospect of permanently restricting German economic and military potential by limiting Berlin's sovereignty over the resources of the Ruhr and the Rhineland and of securing permanent material guarantees against German recovery and aggression was terminated, leaving only a "system of paper guarantees against German hegemony." And, in Bari  ty's analysis, the outcome of the postwar Franco-German contest for industrial supremacy was decided with the failure of French efforts to exploit the advantages confirmed by Versailles and to transfer wealth and resources from Germany to France. (2) The events of 1924 determined, as Maier has insisted, how the costs of the war would be paid. The problems of the balance of payments and the burdens placed on international exchange by the flow of reparations and war debts westward, from Germany to the European victors and to the United States, were alleviated when some obligations were written off, when others were reduced, and when loans from American banks began to recycle intergovernmental payments. And (3), in exchange for their assistance in rebalancing international capital flows, the Americans in conjunction with the British were able to require the separation of the reparation settlement of 1924 from the resolution of both the war-debt issue and the security problem.⁴⁸ In general, Britain and the United States succeeded in imposing on Western Europe their concept of stabilization—based on international competition, the conciliation of Germany, and peaceful change—and supplant a French concept of stabilization—based on Allied solidarity, the coercion of Germany, and the status quo. The Dawes-London settlement was, thus, a considerable victory for London and Washington.

Finally, the settlement of 1924 created a solidarity between the Germans on the one hand and the British and the Americans on the other. This alignment, which was based on U.S. capital exports, had far-ranging and long-lasting political repercussions. Schuker viewed the formation of this alignment, rather than any Franco-German rapprochement, as the central event of the London Conference. Its effect, McDougall has stated, was to reaffirm the economic and political unity of Germany, which had been preserved in the Versailles settlement, and so to leave unaltered the preponderance of German demographic and economic capabilities, which had largely survived the military defeat of 1918. The consequent recovery of Germany to its prewar position as the premier industrial power on the Continent, funded with American loans, outlasted the subsequent withdrawal of credit in 1929–31.⁴⁹ More immediately, the reparation settlement of 1924 opened to Gustav

⁴⁸ Schuker, *The End of French Predominance in Europe*, 343, 386–87; McDougall, "Political Economy versus National Sovereignty," 21; Schuker, *The End of French Predominance in Europe*, 354–59; and Silverman, *Reconstructing Europe after the Great War*, 145–98.

⁴⁹ McDougall, *France's Rhineland Diplomacy, 1914–1924*, 369, and "Political Economy versus National Sovereignty," 21–23; and Schuker, *The End of French Predominance in Europe*, 180, 386.

Stresemann, German foreign minister from 1923 to 1929, the prospect, never fully realized, that Anglo-American pressure on Paris could continue to be mobilized by appeals to detente as the corollary to secure investment and that this detente could include the retreat of French military force from the Rhineland as well as the Ruhr, diminishing France's remaining autonomous source of power and eventually making possible a foreign policy aimed at territorial revision.⁵⁰

In this interpretation, then, the liquidation of the Ruhr invasion and the reparations settlement of 1924 did less to facilitate the reconciliation of Germany or form a Franco-German special relationship than to end the prospect of either and to set an aggressive future pattern for both German intentions and capabilities. The year 1924 thus emerges as the time when the potentialities of the settlement of 1919 were exhausted, as the end of the postwar era, as the decisive shift in the interwar balance of power, as the major interwar turning point. In Trachtenberg's terms, the results of the Ruhr were "to determine the structure of European politics right down to 1939."⁵¹

It seems likely that the issues raised by the new international history of the 1920s will be widely debated among historians of twentieth-century international politics for some time to come. Not only does the place of that decade in the history of the twentieth century merit further reconsideration but the dimensions and the significance of the mid-decade shift in international relations require further clarification. There is basis to question the latter notion that the reparations settlement of 1924 set the pattern for the immediate future. Late in that year French resistance to further travel down the American road to stabilization stiffened as Paris turned away from U.S. proposals for debt settlement and currency stabilization and rejected Washington's overtures to attend a conference on arms limitation.⁵²

French diplomatic recovery was abetted by a change of government in London. By November, as I have shown elsewhere, Austen Chamberlain, British foreign secretary from 1924 to 1929, was meeting with Herriot in Paris and at the League Council session in Rome, both times without the Germans present. There the two ministers coordinated their German policies, and Berlin was soon confronted with the three-pronged, post-Dawes-London, Anglo-French counterattack: the prospect of a British-French-Belgian alliance, the continuation of diplomacy by sanction involving a delay in the beginning of the evacuation of the Rhineland, and the requirement that Germany join the League of Nations without reservations or exemptions and accept permanent League inspection of German disarmament. Although Paris and London dropped their insistence on unconditional League

⁵⁰ For a collection of recent work on Stresemann's foreign policy, see Wolfgang Michalka and Marshall M. Lee, eds., *Gustav Stresemann* (Darmstadt, 1982). And, for analyses of some of the Stresemann scholarship of the 1970s, see Manfred J. Enssle, "Stresemann's Diplomacy Fifty Years after Locarno: Some Recent Perspectives," *Historical Journal*, 20 (1977): 937-48; and Robert Grathwol, "Stresemann Revisited: Review Article," *European Studies Review*, 7 (1977): 341-52. Also see the well-annotated bibliographies in Kimmich, *German Foreign Policy, 1919-1945*; and Peter D. Stachura, *The Weimar Era and Hitler, 1918-1933: A Critical Bibliography* (Oxford, 1977).

⁵¹ Trachtenberg, *Reparation in World Politics*, 289.

⁵² Leffler, *The Elusive Quest*, 113-14.

membership at the Locarno Conference in October 1925, the French scored a double success there, marking a significant recovery from the reversals of 1924. Both Britain and Germany were integrated into the French security system, Germany with a pledge of nonaggression and Britain with a promise of military assistance. French continuation of military occupation thereafter put both Berlin and London at a disadvantage. Throughout the second half of the decade, Paris could and did compel German observance of the Versailles Treaty and could bind London to French policy by threatening to extend their own military occupation to any territory the British evacuated unilaterally.⁵³ In addition to this significant, post-1924 political asset, Paris retained undeniable military advantages—trained manpower, heavy weapons, incipient fortification of the nation's frontiers, and corresponding limitations on German arms, including the prohibition of German militarization of the Rhineland.

How conclusive, then, was the French defeat? In 1924 Paris surrendered the right to re-enact the Ruhr invasion, to apply sanctions without active British support and without the implied permission of the United States. The British, meanwhile, maintained military occupation until 1930 as an effective sanction against German abrogation of Versailles. This mutual renunciation of unilateral action formed the basis of the post-1924 Anglo-French entente. From the decade-long perspective of the 1920s, the London Conference stands out as the decade's most prominent example of London leading Paris down the path of concessions to Berlin. But it also emerges as the beginning of the end of the most serious of the episodic strains on the interwar entente. The years before 1924 were largely the story of unsuccessful German attempts to exploit divisions in that entente. However strained—even at the height of the Ruhr invasion—it never broke. After 1925, Stresemann was frustrated in his attempts to recapitulate the successes of Dawes and Locarno. Throughout the decade, then, disagreements between Paris and London obscured, but did not dissolve, the hard core of mutual interest that bound the French and the British together.⁵⁴

The reparation and security settlements of 1924–25 did not, however, completely restrict French independence. French rights to enforce the Treaty of Versailles unilaterally had been established in legal theory in 1919, but their real political basis was always tenuous. What gave French foreign policy a measure of independence—despite financial dependence—was an army in position on the Rhine. French policy did not lose that advantage in the second half of the 1920s. Berlin's regular efforts to shorten the period of military occupation compelled the German foreign minister to consult with the French foreign minister, Aristide Briand, four times a year (health permitting), but Paris held veto power over concessions to Germany. And Chamberlain supported Briand as the on-location director of Western Europe. This looks less like the end of French primacy than like the perpetuation of joint Anglo-French hegemony. The issue of whether strategies of initiative were accessible to the French diminishes in significance in the face of post-1924 realities of

⁵³ Jacobson, *Locarno Diplomacy: Germany and the West, 1925–1929* (Princeton, 1972), 3–98; and Christoph M. Kimnich, *Germany and the League of Nations* (Chicago, 1976), 52–75.

⁵⁴ John Hiden, *Germany and Europe, 1919–1939* (London, 1977), 50–85; and Jacobson, *Locarno Diplomacy*, 239–50. Also see Marks, *The Illusion of Peace*, 26–54.

power. In international relations, as in national political economy, of the late 1920s, power came less from the ability to initiate than from the capacity to stalemate and to veto.

Even during France's period of financial vulnerability, 1924–26, it retained some significant advantages, although financial duress did compel a tactical change in foreign policy from confrontation to detente.⁵⁵ Then, when French finances were stabilized within two years of the London Conference and Berlin attempted at Thoiry in September 1926 to transform the London-Locarno-Geneva detente into a comprehensive relaxation of the penal and territorial clauses of Versailles, Paris refused, illustrating dramatically the limits of French concessions to Germany. Comparing some of the hypothetical potentialities of postwar French policy with its results seems to make 1924 a year of defeat; on closer examination, however, that comparison shows French policy after 1924 to have been an impressive holding action. The mistake is in expanding the surrender of unilateral treaty enforcement to encompass the demise of French independence in foreign relations and then to term the latter “the end of French predominance in Europe.”

If French defeat has been exaggerated, so have the magnitude of German victory, the extent of German recovery, and the range of German ambitions during the mid-1920s. Berlin made concessions in both the London reparations settlement and the Locarno security agreements of 1925, concessions that resulted in real opposition within Germany and difficulties for Stresemann in securing their ratification by the Reichstag. Dawes-London was a first step in the recovery of German *Gleichberichtigung*, but the policy Stresemann pursued for the next half-decade reveals continued German weakness. Germany had neither the military force nor the social consensus and financial strength to support his diplomatic offensive. It was fueled “with cajolery and unveiled threats of Leftist or Rightist takeovers.”⁵⁶ To label this era “the revision of the peace,” as Sally Marks has done, is to overestimate Stresemann's success. Rather than simple revisionism, the policy of *Westorientierung* Stresemann pursued was a dialectical strategy of opposition and compliance in continuous negotiation with Paris and London. It had as its dual objective modification in the execution of the Treaty of Versailles and preparation for its eventual revision. Continued military occupation after Locarno compelled a policy of accommodation with France, limited Berlin's foreign policy options, and put territorial revisionism in cold storage while debate centered on incremental and symbolic issues, such as how many troops would occupy the Rhineland for how long. Meanwhile, the international steel cartel and trade treaties into which Germany entered, as well as the acceptance of American loans, were moves toward the integration of Germany into a Western-oriented, liberal economic order.⁵⁷

The liquidation of the Ruhr invasion, the Dawes Plan, and the London

⁵⁵ On French policy after 1924, see Clemens A. Wurm's excellent *Die französische Sicherheitspolitik in der Phase der Umorientierung, 1924–1926* (Frankfurt a/M., 1979).

⁵⁶ Marshall M. Lee, “Some Comments on Continuity in Recent German History,” remarks made to the Western Social Science Association, Denver, Colo., April 1977.

⁵⁷ Ulrich Nocken, “Das internationale Stahlkartell und die deutsch-französischen Beziehungen, 1924–1932,” paper delivered at the Conference on Politischer Nationalismus und weltwirtschaftliche Finanz- und Handelsordnung: Konstellationen internationaler Politik in den Jahren, 1924–1931/32, held in Dortmund, October 1981.

Conference were the beginnings of an incremental and protracted resettlement process that continued through 1926. During that time, reparations and war debts, security and disarmament, and the terms of international trade, currency exchange, and industrial relations were recast. This mid-decade stabilization was not unparalleled. As Maier has pointed out, it occurred conjointly with the advent of an internal political economy that was both conservative and corporatist and with a transnational restabilization of capitalism that failed to recognize the potentialities of mass consumption and sustained growth.⁵⁸ Nor did it realize the potential of extending detente to capital-hungry Communism, subsequently explored in the 1970s. The reorientation of Russian policy in 1921, after the failure of both Allied intervention and world revolution, offered the West the prospect of peaceful coexistence based on diplomatic recognition and traditional diplomacy and of the partial reintegration of the Russian economy with the capitalist West based on an exchange of raw materials and markets for long-term loans.⁵⁹ The subsequent Dawes-Locarno-Geneva detente between Germany and the West weakened German ties with the USSR and separated the two anti-status-quo powers. From the perspective of East-West relations, that detente was less a defeat for France than a success for the capitalist powers. Although postwar stabilization centered on Western Europe and the nascent Atlantic community, its potential was not limited to those regions. Recent scholarship therefore leaves to its successors the employment of a more fully international perspective toward the decade as a transition from a pre-World War I, Europe-centered world order to a post-World War II bipolar system including Europe, and it leaves to subsequent work the utilization of more multidimensional analyses to examine the linkages within the national, international, and transnational stabilization package.

The international stabilization of the mid-1920s was partial, tentative, and fragile, and it incorporated latent disaffections, unresolved conflicts, and inherent contradictions. But it was sufficiently comprehensive, complex, and closely linked to the stabilization of internal corporatism and transnational capitalism not to be dismissed as a series of "false settlements" made during "years of illusion."⁶⁰ It was the pivot on which turned some of the more significant developments of twentieth-century international politics. Many of the struggles that had begun before World War I and had continued after the armistice of 1918 were concluded in 1924–26. Mid-decade international stabilization, like national and transnational stabilization, was achieved after the efforts of the revolutionary and evolutionary Left to

⁵⁸ On the simultaneity of internal, transnational, and international stabilization after 1924, see Maier, *Recasting Bourgeois Europe*, 478–84, 542–45, 579–86. The internal compromises on which the tenuous stability of the Weimar Republic rested have been studied more fully than those of other European states for the same period. They were initially exposed comprehensively at the level of parliamentary coalitions in Michael Stürmer's *Koalition und Opposition in der Weimarer Republik, 1924–1928* (Düsseldorf, 1967), and Maier's *Recasting Bourgeois Europe* probes them to the level of interest in group competition. They have been most recently examined in terms of class conflict in David Abraham's *The Collapse of the Weimar Republic*. All three are superior works. A beginning could be made for France with an examination of the terms on which Briand and Poincaré collaborated from 1926 to 1929.

⁵⁹ Teddy J. Uldricks, "Russia and Europe: Diplomacy, Revolution, and Economic Development in the 1920s," *International History Review*, 1 (1979): 62–68.

⁶⁰ McDougall, *France's Rhineland Diplomacy, 1914–1924*, 375; and Marks, *The Illusion of Peace*, 74.

transform Europe had been contained, after the moderate parliamentary center in Western Europe had failed to mediate the conflicts induced by internal and international upheaval, and after the costs of nationalist and unilateral solutions had been demonstrated in the Ruhr. Therefore, the agreements reached in 1924–26 revealed both the conditions that facilitated and those that inhibited the achievement of stability after a decade of war, revolution, and inflation.

Stabilization was achieved on terms that were essentially different from those on which the treaties of 1919 were negotiated. It was based less on defeat, surrender, and domination than on exhaustion, stalemate, complex trade-offs, and hard-fought bargains reached through compromise on all sides. In that they gave Europe a stability that neither the end of the war nor the Treaty of Versailles offered, the agreements of 1924–26 were the real World War I peace settlement. They regulated the conflicts remaining after Versailles, established the order that was challenged in the 1930s, and forecast the terms on which a more durable stability was achieved after World War II.

Reviews of Books

GENERAL

KARL F. MORRISON. *The Mimetic Tradition of Reform in the West*. Princeton, N.J.: Princeton University Press. 1982. Pp. xxii, 440. \$32.50.

In so far as historians engage in a "representation of reality" their work is mimetic. Yet Karl F. Morrison "began the present essay to determine why historians had stood aloof from scholarly debate over mimesis" (p. 399). He has written a history of mimetic thinking from Plato to Dilthey and Durkheim rather than of the literary and artistic practice of mimesis. It is Morrison's central thesis that an element of reform entered into the mimetic tradition beginning with Aristotle. The latter taught, according to Morrison, that mimesis was a basic process of nature and of man, but that human consciousness made corrections of the errors of nature and previous mankind. Although it is arguable whether such a progressist notion can be extrapolated from Aristotle, Morrison correctly derives the application of mimesis to history from early Christian thought, especially that of Augustine. The great myth of Genesis—of man, created after the image and likeness of God and commanded to multiply and dominate the vegetable and animal worlds (Gen. 1:26–27), followed by the story of the Fall and expulsion from paradise (Gen. 2) and man's redemption by Christ's atonement—could either take on the upward *reversion* and deification of Neoplatonism or move toward *augmentation* of the Creation and man's lot within it by corrective human mimesis of the divine pattern, as it came to be interpreted by Christian theologians. Augustine, Morrison holds, viewed the Jews and pagans as victims of a "false consciousness" and the Christians as the true sons of Abraham, and in this dialectical *inversion* drawn from Paul's *subversion* of the old order Augustine "forecast the process of inversion framed by Hegel and reshaped by Marx" (p. 87). (Italicized words are Morrison's categories.)

It is a fundamental argument of this book, and one that historians need to heed, that the most influential thinkers in formulating modern historicisms or establishing a positivistic empiricism retained a preconception of a pre-existing pattern of justice and harmony, to which they saw purposive human actions seeking to conform by some tactic of mimesis. Indeed, Morrison holds, an asymmetry of ideal and actual, mediated by mimesis, is the underlying pattern of Western history. A crucial moment in the functioning of this grand strategy of asymmetrical mimesis comes in the turning of direction in which it is pursued, from upward (or backward) to God and the original justice of the Creation (either the Platonic or the priestly) to forward toward the Christian commonwealth, the rule of the Absolute Spirit, or the classless society.

The scenes depicted (that is, the thinkers interpreted) in this scenario are inevitably selective. After Augustine and Gregory the Great, the founders of the Western Christian order, the Carolingians Radbertus, Hincmar, and Scotus Eriugena are seen to have stressed the augmentation of church and creation in various ways. But the thinkers of the twelfth century, so prolific in mimetic speculation, are passed by. Thomas Aquinas's intellectualized vision of a cosmic order of redemption represents the Scholastics. Ockham and other nominalists are not even mentioned, though they were radically subversive of the Aristotelian-Thomistic synthesis of image and exemplar and as critical in the projection of a nonmimetic view of earthly morals and society as the Reformers, Luther and Calvin, whom Morrison emphasizes. Rather Eckhart and the Rhenish mystics are depicted as having projected a horizontal deification that mingled God with his Creation and blended image and exemplar. Pico della Mirandola and Cusanus represent the Renaissance notion of human multipotentiality, totally ignoring Petrarchan humanism, so important for a cultural and political remodeling of society based on a paradigmatic antiquity (Machiavelli, for example). If the

Reformers and Francis Bacon represent a break away from mimesis, the empiricism of Locke and Hume only concealed their mimetic assumptions of underlying harmonies of human perception and sentiment. But mimesis was restored as the grand strategy of historical progress by Herder and the "four Lutherans": Humboldt, Hegel, Schleiermacher, and Goethe. Mimesis was enthroned in its ancient transcendental splendor by Pius IX but allowed a more worldly manifestation by Leo XIII and John XXIII. John Henry Newman was made a cardinal in his seventy-fifth year despite a deep sense of cultural autonomy that contradicted his conservative orthodoxy. And in the opposing camp of the deified historical dialectic Marx manifested a truly apocalyptic vision of the fulfillment of the dialectical process of self-correcting mimetic history. Finally, the sociologism of Durkheim and the historicism of Dilthey are also seen to retain a paradigm of human unity in the chaos of competing groups and cultures.

Sometimes obscured by over-resort to abstraction, Morrison's scholarship is nonetheless superb—both wide-ranging and profound. His conceptual framework is convincing after hard examination, though not always at first glance. He is hard on his readers, even on one as familiar with much of his material as I had thought myself to be. I do feel he is wrong to dismiss the classical sophistic and rhetorical tradition and to entirely ignore twelfth-century Platonism, the *via moderna*, and the non-Platonic Renaissance humanists as he does. The notion that men mimic paradigms of their own invention put forth by sophists and humanists surely requires treatment in the context of Morrison's conceptions. The hiatuses in the modern period are even vaster. For such a huge subject a second volume, which would have permitted even greater comprehensiveness, would have been very appropriate. As is to be expected in a study of such scope and complexity, one can argue with innumerable details, and some scholars undoubtedly will.

It is also hard to retain a balanced perspective when, as he must, Morrison neglects the antimimetic elements in ancient, medieval, Renaissance, and modern thought. I do not believe Morrison wishes to deny their existence. What he does wish is to show the fundamental importance of the philosophies of asymmetrical mimesis in Western historical thought and historical consciousness, even where its presence is not expected or suspected, as in Hume, Durkheim, or Dilthey. This he has accomplished most effectively—at least for this historian, who, perhaps, did not need that much convincing to begin with.

CHARLES TRINKAUS
University of Michigan,
Ann Arbor

STEVEN J. DICK. *Plurality of Worlds: The Origins of the Extraterrestrial Life Debate from Democritus to Kant*. New York: Cambridge University Press. 1982. Pp. x, 246. \$34.50.

This is a story that profits by Steven J. Dick's telling it *à la longue durée*. Ancient atomist belief in a plurality of *kosmoi*, based on the difficulty of packing infinitely many atoms into one *kosmos*, was refuted by Aristotle, who insisted on a unique natural place for each particle in the universe. A particle moving between universes, and thus offered the choice of many natural places, could never find its proper seat. Atomist cosmology was little known in the Middle Ages, but Aristotle's critique of it became medieval dogma until in the condemnation in 1277 Aristotelian constraints on natural action came to be seen as limits on God's power. John Buridan, William of Ockham, and Nicole Oresme then proposed that God might have made any number of universes, though in fact only one had been created. Shortly after Oresme died, Poggio Bracciolini recovered the text of Lucretius, thus restoring an important vehicle of pluralist cosmology to European readers.

After Copernicus sparked the demolition of Aristotle's *kosmos* the debate shifted from plural *kosmoi* to plural earths. Proceeding philosophically, Giordano Bruno envisioned an infinite universe filled with countless earthlike bodies, while Johann Kepler reasoned empirically, stressing visible similarities between the earth and other celestial bodies and pursuing his analogy to its conclusions: for example, that a moon resembling the earth topographically should also resemble it in being inhabited. Kepler also employed the argument from purpose—there must be Jovians to enjoy the satellites of Jupiter—an argument whose uses were multiplied by new telescopic discoveries. Many of Kepler's contemporaries, Galileo among them, acknowledged through their caution that central theological doctrines (creation, incarnation, redemption) were threatened by such speculations. Likewise, Descartes and his earlier followers were reluctant to press their vortical cosmology to its fullest, even though vortices were excellent models for a plurality of solar systems. Completion of the Cartesian chain of reasoning came finally with Fontenelle, who made the idea of many inhabited worlds accessible to a wide public and also made it theologically safer by speaking of extraterrestrials who were not human—not children of Adam.

Unlike its Cartesian rival, Newtonian cosmology contained no physical ideas compelling a belief in other worlds, but Newton's very success in physics made an active, providential deity so redundant that his pious followers scrambled to construct a Newtonian natural theology in which divine power and

goodness were inferences from a multitude of populated worlds. A generation later, Immanuel Kant also invoked natural theology in propounding galactic star systems and in peopling this grander universe with intelligent life graded according to the analogy of nature. Meanwhile, Eric Engman's little-known Uppsala dissertation (1740) had already exposed the poverty of the cosmological tradition that culminated in Kant. From Plutarch's *De facie* to Fontenelle's *Entretiens* proponents of extraterrestrial life had argued well and reasonably but, Engman showed, not better or more reasonably than their adversaries. Reason could not decide this debate, and sufficient empirical evidence was and still is lacking.

Especially in the earlier chapters, specialists may quarrel with Dick on certain points of fact or may wish for greater depth, but on the whole his book is a triumph of intellectual-historical synthesis and deserves a wide readership.

BRIAN P. COPENHAVER
Oakland University

HUBERT L. DREYFUS and PAUL RABINOW. *Michel Foucault: Beyond Structuralism and Hermeneutics*. Afterword by MICHEL FOUCAULT. Chicago: University of Chicago Press. 1982. Pp. xxiii, 231. \$25.00.

Hubert L. Dreyfus and Paul Rabinow's *Michel Foucault* provides a welcome discussion of developments and transformations in the thought of one of the most complex and influential of recent French theorists. Although their account is often philosophical in orientation and does not systematically focus on the problem of Foucault's attempt to provide a critique of historiography, it should nonetheless interest historians. Indeed, the attempt to track the modifications in Foucault's writings is itself a task to commend the book to anyone seeking an Ariadne's thread through the immense labyrinth spawned by the controversial French master. But this virtue of the book is also in certain respects its limitation.

Dreyfus and Rabinow claim that they are providing "a reading of [Foucault's] work, bearing in mind a certain set of problems, i.e., an interpretation" (p. xv). What they actually provide is less a reading in the contemporary critical sense than a perspicuous analytic schematization illustrated by crucial aspects of Foucault's thought and punctuated by objections to it (often paralleling, in semiauthorized fashion, the retrospective self-criticisms of Foucault himself). In other words, their study follows and attempts to clarify Foucault's own self-interpretation, and it does relatively little to test this self-image against a close reading of the texts themselves with special attention to the actual uses of language in them and the "intertextual" relations between them. Nor is

there any discussion of the rhetorical and stylistic dimensions of Foucault's writing, including its more problematic elements, such as the very opacity and terminological profusion of texts in which Foucault seeks a transparent technical lexicon. (For the historian, an obvious but nonetheless crucial issue in this respect is the extent to which Foucault's rhetoric tends to ride roughshod over the problem of furnishing convincing empirical evidence for his various theses and assertions.) Dreyfus and Rabinow even run the risk of unintentional self-parody when they quote a conceptually rarefied, lyrically diffuse, almost incantatory passage that sets one's eyes atwinkling and one's brain awirling and then proceed to paraphrase or comment on it with unflappable analytic sobriety. Their own use of language, which turns Foucault's dance of the seven veils into serviceable domestic homespun, makes their enterprise a very active appropriation (or "interpretation") of Foucault.

In the process, their own painstaking method of conceptual clarification and parsing of arguments at times illuminates issues in an extremely valuable fashion, but, at other times it harbors unexamined assumptions and questionable tendencies of its own. For example, they convincingly argue that Foucault in *The Archaeology of Knowledge* had no cogent account of the role of discursive rules and their relations to social practices. But they also tend to repeat Foucault's own dubious assumption that texts may always be seen simply as "tokens" covered by "types" of discourse. This assumption is, however, particularly problematic with respect to complex texts, indeed those texts partially serving to institute—but also to contest—what Foucault calls discursive practices (texts, by the way, that include Foucault's own). Dreyfus and Rabinow also tend too readily to deny any direct involvement of natural science in political and cultural issues, such as the domination of nature. Conversely, they see normative and normalizing social order only in terms of insidious, totalizing domination, to the exclusion of modes of sociability and the interplay between norms and teaching by example in sustaining social life. Sometimes confusing as well are Dreyfus and Rabinow's recurrent bouts with the putative problem of the connection between discursive and non-discursive practices. Here they employ the notions of the discursive and the nondiscursive in rather unspecified, even labile ways, and they do less than Foucault to elucidate the relationship of various uses of language to one another and to the activities with which they are, in Foucault's term, "articulated." (Most often they seem, for unstated reasons, to restrict the discursive to the decontextualized theoretical or formal level of language use, but at other times they make it as broad as the use of language in general.)

Dreyfus and Rabinow's entire treatment of the question of Foucault's relation to structuralism is marred by the fact that they rely on an overly formulaic and at times dogmatic conception of structuralism that gives little idea of the movement as an intellectual adventure that attracted a variety of impressive figures and was played out in their highly intricate, sometimes ironic texts. Thus they do not bring to the understanding of structuralism the tense interplay of involvement and distance that Foucault calls for in a "genealogical" account. Nor is there any sustained inquiry into the problem of the relation between structuralism and those partially immanent critiques of it often termed "post-structuralist." By jarring contrast, there are numerous comparisons of Foucault's views with rather uncritical accounts of John Searle's variant of speech act theory and T. H. Kuhn's conception of paradigms—approaches that had little formative role in the development of Foucault's perspective. As a result, one gets little historical or theoretical sense—here one cannot separate the two—of the discursive contexts that (as the authors themselves acknowledge on page 127) were significant for the elaboration of Foucault's approach. These contexts were in good measure shaped by figures whom Dreyfus and Rabinow for the most part do not even mention (Althusser, Barthes, Derrida, Lacan, *et al.*). Dreyfus and Rabinow both follow Foucault's own understandable desire for marginal differentiation from his fellow French superstars and, notably in the first part of the book, treat him in a manner that is most adapted to his reception by a specific Anglo-American audience, particularly one with an interest in the procedures and strategies of analytic philosophy.

Within the context of their own assumptions, however, the book is admirably executed and well worth reading—especially the second half where the pace of the analysis picks up considerably. Quite helpful is the discussion of the recent Foucault and the way in which his current work takes him in directions significantly different from those mapped out in the earlier writings. Noteworthy in this respect is, of course, Foucault's inquiry into the social practices that coordinate power, knowledge, the body, and the constitution of the subject of discourse. Here Foucault makes the argument that repression is not the most general form of domination; sexual liberation may itself accompany the proliferation of the techniques of "bio-power" such as the confession. Hermeneutic and clinical sciences arose to interpret the discourse of the signifying subject, and they found their *frères ennemis* in structuralist sciences that explained the human being as signified object.

Exceptionally acute is Dreyfus and Rabinow's treatment of how Foucault's own earlier projects, especially that of a quasistrukturalist "archaeology of

knowledge," generated internal problems that led him to abandon their more extreme claims. Foucault confronted the same impasses he criticized in the "human sciences." Theoretical binds (such as that besetting the subject-object dichotomy) were combined with the practical neutralization of the detached "archaeological" observer who was untenably situated both inside and outside history and unable to enter the debates that agitated it. The resultant dilemmas induced Foucault to work out what Dreyfus and Rabinow call an "interpretive analytics" that for them goes beyond (yet retains selected aspects of) structuralism and hermeneutics in its pragmatically based attempt to write a "genealogical" history of the present. (The book also contains an extremely valuable essay by Foucault himself that is one of the very best introductions to his recent work.)

One may certainly follow Dreyfus and Rabinow in recognizing the historiographical and political significance of Foucault's current attempt to extend Nietzsche's exploration of will-to-power dynamics. But the question they do not raise is whether Foucault tends over time to go from one extreme to another: from the insistence upon the operation of abstract archaeological rules to an emphasis upon the anomic play of power. Obscured in this movement is the issue of the actual and desirable role of legitimating norms in both authorizing and resisting various uses and abuses of power—norms that cannot be seen exclusively either as derivations of formal rules or as juridical masks for the myriad assertions of omnipresent power. The recent Foucault is engaged in an important critique of the principle of sovereignty, be it the sovereign will of divinity or of its secular surrogate, the people. But the tendency to occlude the problem of normative practices not derived from a sovereign source (including the archaeological rule) leads Foucault to interpret society and politics predominantly in terms of power, strategy, and tactics—a tendency that may well reinforce some of the most questionable features of modernity.

DOMINICK LACAPRA
Cornell University

JOAN LEOPOLD. *Culture in Comparative and Evolutionary Perspective: E. B. Tylor and the Making of Primitive Culture*. (Beiträge zur Kulturanthropologie.) Berlin: Dietrich Reimer. 1980. Pp. 183.

In this scholarly contribution to the "Beiträge zur Kulturanthropologie" series, Joan Leopold discusses and evaluates the sources familiar to Edward Burnett Tylor (1832–1917) in his writing of *Researches into the Early History of Mankind and the Development of Civilization* (1865) and *Primitive Culture* (1871). Six

themes from Tylor's books are examined: (1) the preparation, writing, and postpublication reviews of the two volumes; (2) evolutionary theory; (3) independent invention, inheritance, and diffusion as well as concepts of psychic and environmental unity; (4) survivals; (5) methodology; (6) the concept of culture. One-third of Leopold's study is devoted to notes, which include bibliographic citations. The four pages of photographs include pictures of some of Tylor's contemporaries and pages from his notebooks. The frontispiece is a photograph of Tylor that was taken around 1860 and a photograph of his wife. The book does not contain a subject or author index.

Leopold asserts that Tylor's anthropological contributions are less the products of the English empiricist, positivist, and materialistic traditions than a summing up of nineteenth-century thought concerning conflicts of diffusionist and evolutionary perspectives, monogenism and polygenism, and synchronic comparative and diachronic comparative historicist methods. These issues are scrutinized under the light shed by the lamps of archaeology, geology, paleontology, biology, linguistics, folklore, history, and geography. Tylor's application of the new ideas from natural history and social philosophy, however, came to be interpreted in different ways by his contemporaries and successors in Europe and America. Furthermore, the degree of attention accorded Tylor's "key themes" varied among his reviewers within the period of his own lifetime. Appreciation of his methodological approaches earned him widest recognition in the early years of the present century, his concept of culture finding its most receptive audience after the Second World War.

Culture history was, for Tylor, a subdivision of history, itself a subdivision of science. He thought that from accumulation of ethnographic data would emerge general laws. Not surprisingly, given this emulation of the natural sciences, Tylor found models for culture history in the uniformitarianism of geologists and astronomers, in the zoogeographical maps of biologists, and in the application of scientific principles of statistics and probability. But Tylor was alert to the role of individuals in the spheres of inventions, opinions, and innovations of customs. Leopold suggests that Tylor was able to maintain a holistic view of the group and the individual, as both levels of behavior would yield mutually consistent results. In her opinion, Tylor was supporting a common attitude of Enlightenment and Utilitarian thinkers while rejecting the Romantic and positivist elements of nineteenth-century sociology.

The reading of these 124 pages of text demands concentration by scholars familiar with the history of anthropology and the social thought of the last

century. This is not a book for beginners in the realm of intellectual history, and relatively obscure authors of the eighteenth and nineteenth centuries appear in the pages with little or no introduction. Leopold deserves high praise for this effort that, she tells us, will provide background for a forthcoming study of Tylor's treatment of the actual content of culture itself.

KENNETH A. R. KENNEDY
Cornell University

JOHN A. ARMSTRONG. *Nations before Nationalism*. Chapel Hill: University of North Carolina Press. 1982. Pp. xxxvi, 411.

John A. Armstrong's *Nations Before Nationalism* examines the forces that shaped ethnic identity in Europe and the Islamic world. Ethnicity he defines as attitudinal—an affirmation of the superiority of the group excluding others, a symbolic and mythic consciousness of solidarity against outside forces. Chapter by chapter he analyzes the nomadic or sedentary experiences, urban organization, imperial polities, elites and bureaucracies, churches, and languages that gave rise to ethnic consciousness. Woven into each chapter is a discussion of the basic differences and antagonisms among Islamic, Eastern European, and Western European civilizations.

Chapters 2 and 3 argue that nostalgia for a nomadic or sedentary past is the basis of the differentiation of civilizations. Arab concepts of kinship and Western territorial ideas, each reinforced by Islam or Christianity, led to opposing genealogical and spatial concepts of political identity. Imperial polities (chaps. 5–6) consolidated the differences. Ancient Mesopotamian myths of the earthly kingdom as a reflection of heavenly rule, and related ideas of a universal polity, were transformed by later Islamic, Byzantine, and Western European societies such that Islam assigned universality to religion and made no concession to the legitimacy of regional polities, whereas Western European rulers were able to use the division of authority between popes and emperors to legitimize a multiplicity of small territorial regimes. Differing imperial administrations and elites were equally important. Islamic bureaucracies were too heterogeneous and segmented to create and suffuse a myth of ethnic identity in a single language. Byzantine bureaucracy, committed to the Greek language and imperial unity, led eventually to a Greek national identity. The Western empire failed to create a supranational administrative elite; therefore, the multiplicity of European nationalities.

Analogously, Muslim cities, segmented communities, lack of civic consciousness, and the absence of a

spatial symbolism for the existence of an urban community all militated against the formation of ethnic or territorial consciousness on the basis of shared habitat. So too, Eastern European cities, often islands of foreigners such as Germans or Jews, did not share the law and culture of the countryside and could not be fully assimilated into gentry-controlled societies. Western European cities, however, inherited the ancient civic ideal in which *polis* and *patria* were equated. Both Mediterranean and Northern cities, the one integrating surrounding rural populations, the other generating burgher communities, fostered myths and allegiances that were a model for later state polities.

Conflict between civilizations also defined ethnic boundaries. "In the Danubian basin, the Balkans, Asia Minor, the Caucasus, and the Black Sea steppe . . . the clash of Islam and Christendom constituted a major factor in the emergence of special types of ethnic identity" (p. 92). The competition of Eastern and Western churches had a similarly vital role in the creation of ethnic boundaries and ethnic consciousness in Eastern and Central Europe. The conflict of Greek and Latin churches from the Adriatic to the Baltic and the formation of a Catholic-Orthodox frontier helped to shape Polish, Lithuanian, and Russian identity. Churches, Armstrong points out, are particularly potent in generating ethnic identities, for missionary activity and sectarian disputes require the use of a common language. Ethnoreligious identity is as potent as ethnolinguistic identity. Armstrong, however, comes to the interesting and surprising conclusion (chap. 8) that ethnic alignments arise without much attention to language and that language identity and consciousness are contingent on political or ecclesiastical developments.

Such is the general argument of this complex book, though the bare outline does not convey the interest of the in-depth discussions of Russian, Magyar, Polish, and Habsburg identities; the typology of city forms; the analysis of language and religious identity; and other topics. In general, the discussions of Central and Eastern Europe seem to be the best informed and most provocative. Western Europe is treated in scant detail. As an Islamic historian, I find the insistent distinction between Islamic and Christian civilizations exaggerated. If Islamic tribalism, religious universalism, segmented cities, bureaucracy, and other factors militate against the formation of ethnic identities, how are we to account for the emergence of Arab, Persian, and Turkish ethnicity and the formation of national states in Morocco, Tunisia, Egypt, Turkey, and Iran?

There are other significant flaws. The prose style is stilted and abstract, and the book is awkwardly organized. It is useful, even necessary, to read the conclusions to each chapter before the whole so as to

understand the purport of the historical arguments. There is also a deeper methodological problem. Although the diagnosis of the forces involved in the formation of ethnic identity is rich and persuasive, the book does not allow us to test the validity of its propositions. Since the discussion of Russian, Austrian, Polish, and other European ethnicities is scattered chapter by chapter throughout the book, and because different themes, such as the influence of church and language, are illustrated by different examples, it is impossible to understand any particular case as a whole or to evaluate the relative importance of the formative factors. Even though Armstrong has given us a complex vocabulary for understanding the formation of ethnic identities, no case is analyzed deeply and consistently enough for the historian to say that he understands why any particular country has the identity it does. The book cries out for an integrated study of each region and country so that the relevant considerations can be assessed in context.

IRA M. LAPIDUS
University of California,
Berkeley

V. G. KIERNAN. *From Conquest to Collapse: European Empires from 1815 to 1960*. New York: Pantheon. 1982. Pp. 285. \$16.95.

Genocide, slavery, racism, imperialism, war: these categories of interethnic relations we would most like to forget are not pleasant for historians to explore. V. G. Kiernan's latest book valiantly confronts a topic ugly on three counts—the gore it caused, the racism it revealed, and its role in fastening domination over such a large part of the globe. His work surveys the entire sweep of colonial conquest and repression during a century and a half. It is organized mainly in a chronological manner; most sections are on campaigns in various theaters, but some touch on military hardware, ideologies associated with conquest, and impacts of such violence on European culture.

Structured theorizing about the causes and nature of colonial conquest is entirely lacking here. Kiernan's method could perhaps be described as pointillist: an agglomeration of capsule accounts of campaigns and battles, sometimes simply piled one on another and sometimes adorned with a remark relating the episode to the general history of the colony or to the motivations of the soldiers—the latter based partly on Kiernan's deft use of scores of diaries and autobiographies.

Anyone still under the misapprehension that until 1945 imperial conquest was only a matter of a few not very serious wars, or that the peoples involved

yielded at the first touch of the conqueror's sword, should be disabused by the gruesome and long list of campaigns of conquest and repression in this book. Another valuable contribution here is the wealth of detail Kiernan provides. It is good to be reminded that "loot" is a Hindu word, that the "dumdum" bullet gets its name from the Dum-Dum arsenal (near Calcutta), that Göring was the son of a colonial governor, that there actually was a Colonel Shrapnel, and that in the 1911 Libyan campaign Italian forces used radio. It is a pity, however, that Kiernan's need to be as comprehensive as possible means some campaigns have to be dealt with in as little as one sentence.

A fine feature of this work is Kiernan's highly evocative if somewhat detached comments, for example, those concerning the lust for promotion and prize money that activated many officers in charge of determining war or peace. Such comments are never as somber as his subject matter might lead one to expect; in fact his style is matter-of-fact verging on the sarcastic—perhaps just as well since his assignment involves so much gore. Take it or leave it, he seems to be saying, when assuring us that without liquor "the [British] empire could not have been won" (p. 129). Speaking of an Afghan victory over the British, Kiernan says "Britain was saved from a further calamity by the devoted Scottish orderly who rescued a young surgeon, Dr. Watson," and gives as his reference *A Study in Scarlet*.

Stripped to their essentials and viewed *en masse*, colonial conquests and pacifications are revealed for the misbegotten, mean, and maladroit actions they were. Yet there remain questions we would like Kiernan to address. Is there any way of investigating his notion that in at least some cases the violence perpetrated by conquerors may have been less costly, counterfactually speaking, than the killings of some natives by others, killings we might have expected in the absence of colonial conquest? Are the conquests themselves only the cutting edge of imperialism, or do they constitute a sort of independent variable? Were violence-prone soldiers and governors often somewhat unbalanced (as Kiernan says in one case), or for the most part as stable as you and I?

The costs of colonial conquests may never be paid. "In January 1979," Kiernan tells us, "the centenary of Isandhlwana was celebrated by Africans in London" (p. 226). Other offshoots of these dragon seeds he finds in the militaristic style of governments in many Asian and African countries. The debits cannot even be totaled as yet, since "colonial wars are still going on, openly or disguised" (p. 230).

MARTIN WOLFE
University of Pennsylvania

AMOS PERLMUTTER. *Modern Authoritarianism: A Comparative Institutional Analysis*. New Haven: Yale University Press. 1981. Pp. xiv, 194. \$17.50.

In his brief but intellectually ambitious new book Amos Perlmutter offers a synthesis and a typology of modern authoritarianism, the typical political form of our century. Drawing on a wide range of authoritarian systems, though focusing most intensively on the Soviet Union and Nazi Germany, Perlmutter is more concerned with the functioning of these systems than with their origins. The work is thus an exercise in methodological political analysis rather than historical exegesis.

Indeed, Perlmutter boldly challenges and criticizes the general theory of totalitarianism, particularly as shaped by Talmon and Arendt, for concentrating on ideological-psychological explanations of the sources of totalitarianism. This general theory is unsatisfactory to him because it is concerned not with explanations of modern authoritarian regimes but with the metaphysical origins of mass manipulation and propaganda techniques. Perlmutter joins other critics of the concept of totalitarianism who do not find it helpful for political understanding. By concentrating on the existential views of Arendt, however, and largely neglecting the more institutional work of Friedrich, Franz Neumann, and Ernest Frankel, he unduly neglects the full ramifications of the theory.

Perlmutter's own preference is for analysis of the institutional structure and those dynamics of authoritarian systems that can best be compared by such analysis. He distinguishes modern authoritarianism from traditional forms and from historical tyrannies that were ruled by individuals or oligarchies but lacked mass support. The modern form is the authoritarianism of mass movements, and it is characterized by some form of mobilization to sustain the authoritarian state as well as by political elites, popular support, and specialized political structures and institutions.

The nexus of political power in authoritarian systems is a triad of party, state (including military and bureaucracy), and political police. In the different types of authoritarian regimes, some of which are institutionalized like Communist and Fascist systems and some of which, like corporatist and praetorian regimes, are not, the relationships among and the significance of these three forces vary. Authoritarian systems use what Perlmutter calls parallel and auxiliary structures for purposes of domination, mobilization, and influence. Unable to accept the competitive procedures of democratic countries, these systems rely on forces like the mobilizing single party, a military ruling group, political police, praetorian guards (such as military

squads), and militant groups (such as Red Guards or youth movements). Structures of this kind check the traditional political institutions of the modern state and safeguard the regime's control over society.

Perlmutter draws heavily on the empirical work of scholars of the various authoritarian systems but is not loath to draw his own conclusions. This is especially so in his criticism of development theory, which fostered the belief that development would lead to a system of limited pluralism. This is a tough book, both in the force of its intellectual argument and, especially in the long section on definitions and models, in the demands made on the reader. But it will take its place as one of the most insightful and comprehensive works among the studies of authoritarianism.

MICHAEL CURTIS
Rutgers University,
New Brunswick

TREVOR I. WILLIAMS. *A Short History of Twentieth-Century Technology, c. 1900—c. 1950*. New York: Clarendon Press of Oxford University Press. 1982. Pp. xix, 411. \$25.00.

Trevor I. Williams attempts here to synthesize and popularize volumes 6 and 7 of Singer *et al.*, editors, *A History of Technology*, in the manner that the 1960 *Short History of Technology* (written with T. K. Derry) reworked volumes 1–5 of the larger work. *A Short History of Twentieth-Century Technology* covers “roughly the period of 1900 to 1950,” though it is “an independent work designed, within a fairly modest compass, to give the general reader a broad conspectus of the way in which technology developed in the first half of this century and a glimpse of the social, economic, and political factors that influenced it.” A final judgment must therefore turn on the book's suitability for a general audience and on its scope, since Williams's command of the history of technology has already been established by his editorial work on the later volumes of the Singer compendium.

The earlier short history of the Agricultural and Industrial Revolutions, carrying technology's saga up to about 1900, has reached a general audience, so perhaps this story of the second Industrial Revolution will too. But there are many undefined terms and quite a few technical passages, some of which (for example, on uranium [pp. 53–56] and on polymers [pp. 140–45]) would have to be carefully explained in lectures if Williams's presentations were to be used as classroom material. There are some stimulating passages concerning the necessity of conforming society to technology (for example, the construction of the electrical grid [pp. 64–69])

or the other way around (the intangible effects of tastes in clothing on artificial fiber manufacture [pp. 148–53]). The change from the earlier volume's basically chronological framework to a topical organization was no doubt inevitable, but it has reduced readability. In addition, the usual progression of topics from technologies affecting energy and primary products through those influencing manufacturing and consumer products is oddly interrupted in places (with agriculture, for example, separated from food). The resulting episodic quality of *A Short History of Twentieth-Century Technology* contrasts sharply with the smooth flow of the companion volume.

Limiting the scope to the 1900–50 period would, if it had been followed faithfully, have been the equivalent of stopping a diplomatic history while colonies were in flower. Fortunately, Williams extends “a little beyond 1950” into the 1970s when he wants, so that nuclear power generation, space flight, and computers get in. Still, with the full force of microelectronics in the wings and DNA nowhere to be found, the title can only be misleading and the product less interesting. Moreover, the epilogue's claim that “our survey has been on a global scale” refers, in reality, to far too few European and North American examples in a work whose bibliography is 80 percent British, whose photographs come from Commonwealth countries, whose explanation of the origins of the Manhattan Project does not include the Einstein letter, and whose contention is that World War II began on September 3, 1939. The result is an old-fashioned view that, by its choice of scope in both time and place, avoids discussion of crucial new technologies and major changes in the context through which they are diffused. Even English-speaking readers will thus find themselves referring to Kranzberg and Pursell's work for the stories of newer technologies and to less simplistic sources for the effects of technology on industrial development. To put it bluntly, this is an argument for laissez-faire, technological-fix industrialism, and one that is less well argued that it might be. It erects a weighty philosophy on too narrow a base.

Had Williams not set out to add up his minihistories of our tools so as to illuminate policy and philosophical issues, such criticisms would be unfair. Yet the introduction and epilogue amount to an assertion that the technological society's main problem is social lag: “Patently, and disappointingly, progress in technology has not been matched by progress in social relations. . . . [S]ociety generally—whether technically oriented or not—failed to adapt itself to a world in which both material and moral values were changing far more rapidly than ever before in human experience. Taking the position as it was at mid-century, and, as it still is, we may

appropriately conclude with a further sentence from A. V. Hill's address to the British Association: 'The forces of good and evil depend not on the scientist but on the moral judgment of the whole community' " (pp. 399–400). Such assertions risk diminishing the solid history in this book, and they burden Williams's sensible remarks about the conditions for innovation and the consequences of technology with unnecessary ideological baggage. To raise and dismiss Rachel Carson and appropriate-technology advocates in two pages harms rather than helps efforts to put technology's history in perspective. It suggests that the history of technology, unlike its diplomatic and political cousins, is still so insecure that it mentions only winners.

THOMAS J. KNIGHT
Pennsylvania State University

LESTER S. KING. *Medical Thinking: A Historical Preface*. Princeton: Princeton University Press. 1982. Pp. vii, 336. \$19.50.

Over the years Lester S. King, a pathologist, historian, and medical editor, has made valuable contributions to our knowledge of the medical past through his articles, books, and translations. Although the focus of his work is usually the eighteenth century, he traces his information backward and forward in time to place it in proper context. Many of his writings are well suited for classroom use, providing a good introduction to the history of medical practice and medical thought (for example, *The Growth of Medical Thought* [1963], *The Medical World of the Eighteenth Century* [1958], and *A History of Medicine* [1971]). But his latest book, *Medical Thinking*, like its immediate predecessor, *The Philosophy of Medicine: The Early Eighteenth Century* (1978), falls short of his earlier works.

Today's textbooks, King points out in the preface to *Medical Thinking*, omit discussion of topics considered essential by medical writers of the sixteenth, seventeenth, and even the eighteenth centuries: "the nature of health and disease, the best way to classify diseases, the meaning of [clinical] 'signs,' the nature of causation" (p. v). These "foundations of medicine" have been, until the past few years, relegated to the domains of philosophers and historians of science. In other words, few future physicians are provided the opportunity to consider these problems during their years of training. King intends his new book "to hasten [the] process of revivification" and at the same time to further the cause of medical history by demonstrating "the essential unity of medical thinking throughout the centuries" (p. v). Too many physicians, he argues, including some who write for both medical and lay audiences (for example, Lewis Thomas, author of

The Medusa and the Snail [1979]), reject study of the medical past because it seems so archaic, so useless to present needs. But deeper scrutiny reveals that physicians have been asking the same kinds of questions for centuries: What is wrong with my patient? How can I identify the problem? How do I treat it? How can I prevent it in the future? What is its course? How do I know I am correct? It is only the answers to these questions that have changed. The basic patterns and language of medical thought, King asserts, have remained constant.

The author discusses in some detail a number of these fundamental medical concepts. In scholarly fashion King looks at the origin, development, usage, and misunderstanding of such ideas as signs and symptoms, diagnosis, classification, disease and health, facts, scientific method, and scientific medicine. He ranges all over the map and the time line of Western civilization to draw his examples. It is emphatically not a traditional history. Herein lies the book's strength and also its weakness. It is too fragmented, a danger the author acknowledges in his preface but fails to correct. Medical students and physicians, his primary audience, and even trained historians unfamiliar with the history of science and medicine, will find too many unexplained and unfamiliar names and ideas, and too much skipping in time and place, to gain a secure grasp of many of the historical references. In addition, the chapters are only loosely connected, so that concepts or ideas introduced in one are rarely utilized for further development in another. At times, the point of a section, chapter, even of the entire book, becomes unclear as King uses too many words, disparate examples, and unnecessary digressions. He sometimes belabors what appear to be basic, obvious ideas.

Medical Thinking does, however, contain King's unique perspective on the medical world, past and present, which has made his writings so useful and enjoyable to readers. The chapters on tuberculosis, signs and symptoms, and scientific medicine are among the best in the book. Historians, especially those who have not previously read King, should sample *Medical Thinking*.

TODD L. SAVITT
School of Medicine
East Carolina University

ANCIENT

PETER KRENTZ. *The Thirty at Athens*. Ithaca: Cornell University Press. 1982. Pp. 164. \$17.50.

Peter Krentz's work, originally a Yale dissertation of 1975, is a narrative of the events leading up to and involving the regime of the Thirty Tyrants in Ath-

ens near the end of the fifth century. Krentz supposes such a narrative possible because of the relatively full source material for the period. By comparison with that of other decades in Greek history, the material is fairly good: whether it is adequate for a satisfactory reconstruction, however, may be doubted. Sometimes the sources are simply mysterious: Why does Aristotle (*AthPol* 38.2) mention by name only one victim of the board of Ten who succeeded the Thirty, Demaretus? Why does no other source mention him? Who was he? The sources seem almost unanimously written from tendentious points of view, especially anent the moderate Theramenes, and, when there is radical disagreement among the sources (as there frequently is about the details of and the chronology of the actions of the Thirty), it is extremely difficult to determine whether tendentiousness, ignorance, ideology, or other factors are involved. Indeed, one sometimes has the desperate feeling that the most satisfying treatment would be that of a film script or novel (a feeling that Krentz seems to share when he refers to an unlikely reconstruction as a "scenario" and instructs the reader to "imagine that the course of the negotiations between Theramenes and Ly-sander went somewhat as follows" [p. 39]). There are in general too many statements qualified by "probably," "possibly," or "might have": the use of such vocabulary is honest, but it does not inspire belief.

The book may be said to have two main theses, that Aristotle's account is mainly derived from the Oxyrhynchus historian and that at least some of the Thirty consciously intended to model their regime on Spartan institutions. There are no testable arguments for or against the first: since Aristotle is known to have used Androtion and since the latter wrote about the period, he remains the likeliest candidate as Aristotle's source here.

The second thesis is equally doubtful. That there were Laconophiles among the Thirty can hardly be doubted; that a specifically Spartan (as distinct from a more general oligarchic) ideology is reflected in the institutions of the new regime could only have been proved if the Thirty had had more time to implement more fully whatever designs they had. The parallels between the institutions of Sparta and of the Thirty are few: on the Athenian side, there is nothing corresponding to the ephorate; on the Spartan side, nothing corresponding to the Council of Five Hundred nor to the Council of the Areopagus.

The presentation is marred by some errors in detail: the neologism *analogeis* is presumably a compound of the noun *katalogeus* and the verb *anagraphô* (p. 25); the suggestion attributed to Sandys was actually made by Newman (p. 57, n. 1); the plural of *mastix* is *mastiges* (p. 60); the discussion of the laws of

the Thirty on page 76 does not quite square with that on page 61. *AthPol* 35.1, 36.1–2 could not have been borrowed from Xenophon (p. 147); Krentz cites Plato as the author of the *Seventh Letter*, as he may have been, and Aristotle as the author of the *Rhet. ad Alex.*, as he certainly was not.

JOHN J. KEANEY
Princeton University

E. T. SALMON. *The Making of Roman Italy*. (Aspects of Greek and Roman Life.) Ithaca: Cornell University Press. 1982. Pp. xi, 212. \$22.50.

This is a contribution to a series that aims at more or less learned popularization and probably reaches an audience made up largely of students. What E. T. Salmon attempts to describe and, at least according to the blurb, explain is the Romanization—in various senses—of the whole Italian peninsula between the fourth century B.C. and 14 A.D. This is a large and worthwhile task, which no historian of any nationality has really undertaken on the requisite scale. And in this book, too, the requisite scale is lacking, since the author usually starts with what is elementary but allows himself a total of only about sixty thousand words (main text) for the entire subject, with the result that he seldom has the space to make the book interesting to an informed reader-ship.

It is of course useful to have available a sound account of these matters, and Salmon has at least accumulated a fair quantity of accurate information. But the opening chapter on "The Peoples of Italy c. 350 B.C." is not much more than a gazetteer, creating from the outset the expectation that everything is going to be cursory. The rest of the book is divided into chronological chapters, which quickly take the story down to Augustus without providing any special illumination. The author is well informed about colonization in Italy (he wrote a book on that subject in the same series) and clearly has a close personal acquaintance, hardly rivaled among the living, with large areas of Oscan Italy. Also very salutary is his sense of the Romanization of Italy as a long, gradual, but not steady process continuing over many centuries; no one is less inclined to make the mistake of equating constitutional Romanization with the Romanization of cultures and loyalties.

Yet many things go wrong. Within the author's known limits and chosen aims, one must still complain that the Etruscans (whose culture is for most people the most interesting in pre-Roman Italy) get little attention; the work of the many excellent young Italian scholars who work on Hellenistic Etruria is largely ignored (Salmon's bibliography on Etruria and on northern Italy is generally very capricious), and it is feebly and untruly said (p. x)

that the Etruscans need no plates because such illustrations are easily available to the readers of this book. Crucial issues are sometimes skimmed, including the underlying causes of the Social War—a subject about which the author has written interestingly on other occasions. All sorts of important distinctions are persistently blurred: for example, Salmon treats the inhabitants of Cisalpine as “Italians” at dates in the third century B.C. when not even the Romans claimed the Gauls and others in that region as Italians (p. 93). Vergil does not make Jupiter predict that he will unite all the Italians (p. 139), but all the Latins (*Aeneid* 12, 837), a quite different proposition. But what is really worst of all is a kind of Pangloss approach to the realities of Roman expansion: one should not exaggerate, for Salmon is now more alert to economic factors in Roman imperialism (p. 98)—but still there remains a decided tendency to emphasize “fusion” (p. 3) rather than the persistent Roman urge to seize assets and assert control. Such is still the fascination of the victorious cause.

WILLIAM V. HARRIS
Columbia University

PETER GREENHALGH. *Pompey: The Roman Alexander*. Columbia: University of Missouri Press. 1980. Pp. xix, 267. \$20.00.

PETER GREENHALGH. *Pompey: The Republican Prince*. Columbia: University of Missouri Press. 1981. Pp. xv, 320. \$20.00.

Widespread interest in historical biography has been reflected in a number of recently published lives of Roman leaders. Pompey has been treated in several able studies of late (for example, John Leach, *Pompey the Great* [1978] and Robin Seager, *Pompey: a Political Biography* [1979]); but Peter Greenhalgh presents the most detailed account in his two-volume study. (The two-volume format is merely for convenience. Volume 2, although published a year later, assumes volume 1 and continues the narrative in 59 B.C. without a break.) This is not psychohistory or institutional history portrayed through an individual. It is a straightforward, chronological record of the career of Pompey as Roman leader: from his origins and early prominence under Sulla, through his pirate and Eastern campaigns, to the First Triumvirate and civil war with Caesar. Both the military and the political aspects are fully developed. The five maps are excellent, as are the illustrations. Two appendixes on technical questions and a full bibliography in each book aid the historian. I noted only two misprints in these admirably produced volumes.

In his preface, Greenhalgh states that he tried to

write for both the general reader and the specialist. He has succeeded remarkably well. The educated layman should be especially well served. Greenhalgh assumes little technical knowledge. He explains traditions, institutions, offices. If Latin terms are used, they are fully defined. He draws apt modern parallels. He quotes ancient authors in modern, lively translations. His own style is highly readable—though overly given to metaphors.

This success in writing dramatically to capture the interest of the amateur may lead the professional historian to judge that Greenhalgh exceeds proper limits. Beyond men's actions, Greenhalgh interprets their motives, intentions, and feelings. Colorful adjectives and adverbs intensify the roiling fury of the mob, darken sinister plots, intimidate the craven weakling. Although Greenhalgh reads the sources carefully, he is enthusiastic about reading behind the sources. And when the sources are incomplete, Greenhalgh fills the gaps to keep his account balanced and readable; but the distinction between source and speculation is not always clear. The notes, too, provide little comfort for the cautious historian. In order to make them unobtrusive, specific notes are not identified in the text. Rather, they are grouped, according to topics in each chapter, at the end of the book. Although the ancient and modern sources are listed and appraised, their concentration and compression in the notes are daunting to all but the most determined. Despite the notes and a brief introductory chapter on ancient writers covering the period, discrimination among the sources and evaluation of the information is often difficult. And the complexities of problems and variant interpretations rarely intrude into the lively tale. Again, writing for the general audience has prevailed over writing for the specialist. Certainly writing for varying levels of readers is a difficult task, but even the amateur may be ill served by oversimplification.

Like many a biographer who has lived long with his subject, Greenhalgh feels strong sympathies for Pompey. Through the welter of complex military and political struggles, he keeps his focus on Pompey and consistently finds him a brilliant general, an astute politician, a would-be republican, a loyal colleague. In every battle, even the civil war campaigns and Pharsalus, Pompey is skilled and judicious beyond his adversaries. In the political turmoils in Rome, Pompey is more sophisticated and aware than he is usually given credit for. He is the initiator and guiding force of the First Triumvirate. His withdrawals from the political scene are prudently calculated for his own well-being and that of the state. He is fair to Caesar until the Rubicon, trusting him as his henchman. His problems in the civil war are due to the unwise senatorial councilors around him to whose pretensions he bends for the

sake of the Roman constitution. Claims that he failed militarily and politically stem from hostile sources, not from fact.

As Pompey is praised, his foes, critics, and even friends are correspondingly diminished. Cicero, especially, is roughly condemned as a straw man, pusillanimous and miscalculating. But Caesar is the most discredited. His own *Commentaries* are used as evidence of his self-glorification and his mistakes. A not untypical judgment is given in Greenhalgh's account of Pompey's legislation in 52 B.C.: "The vermin which thrived on corruption and disorder fled from the relative sterility of Pompey's Rome to find a more congenial host in Gaul, and the effect of this accumulation of parasites on Caesar's already hyper-thyroidal condition was a dangerous stimulation of the persecution-complex which he began to indulge as convenient justification of increasingly anti-social behaviour" (vol. 2, p. 91).

Despite the limitations imposed by his overt sympathies and popular style, Greenhalgh's work is valuable. He has written a study based on thorough research that has used every shred of evidence. He has thought through the causes and implications of events with mature judgment and makes some very insightful appraisals. The political struggles have fresh interest as seen from Pompey's vantage point of republican moderation within realistic bounds. Three topics deserve mention as remarkably well handled. The social impact of the Pompeian theater complex is well presented. The Eastern settlements after Mithridates are meticulously traced out, providing unwonted clarity to boundaries, administrations, and Pompey's rationale. And—Greenhalgh's finest skill—the many military campaigns, their purpose, geography, strategy, action, and outcome, are developed in full and clear detail.

In the historical competition in which short shrift is too often the lot of the loser, Pompey has found an able spokesman who gives him a sympathetic, informed rehabilitation.

ELEANOR G. HUZAR
Michigan State University

RICHARD P. SALLER. *Personal Patronage under the Early Empire*. New York: Cambridge University Press. 1982. Pp. x, 222. \$39.50.

No one has ever doubted that patronage was an important element in Roman imperial society, but there has been a tendency to take the institution for granted, much as was the custom of gift-giving in Homeric society until M. I. Finley's pioneering work forced us to acknowledge the essential "otherness" of early Greek social practices. In this brilliant study of the early empire, Richard Saller performs a

similar service for the much-neglected practice of personal patronage. His work is sociological history at its best, and, although the weaknesses of the approach are also occasionally apparent, Saller has masterfully combined appropriate topic and appropriate technique.

The world described by Saller is one that is radically different from our own and more akin to the premodern states of China and the Near East that he uses for comparative purposes. We must disabuse ourselves of the comfortable notion that we have a natural or instinctive understanding of such familiar literary figures as Cicero, Seneca, or Pliny and appreciate that they subscribed to a social system that was, in many ways, very alien to our own. Unlike the modern world where impersonal private agencies or public bureaucracies supply the needs of the citizenry, the imperial administration of the Roman world was minimal and distant, requiring the intervention of mediators between rulers and ruled. Patronage handily survived the collapse of the republic, and it would be anachronistic to imagine that the institution shrank in the early imperial period as the result of the conscious creation of abstract, meritocratic criteria for the selection and promotion of officials.

Contrary to modern sensitivity to the primacy of objectivity and impartiality in administrative affairs, the Romans, within certain limits, found nothing objectionable in the practice of patronage. As an ideology of exchange relationships, patronage permeated the language of the republic and empire and was used to describe and conceptualize social roles, relations with friends, with families, and with the gods. It was employed equally in private and public discourse. The author is at his best in accounting for the psychology underlying such patronally related customs as morning salutations and legacy hunting, and explaining certain stylistic usages in the language of inscriptions and letters of commendation.

The day-by-day working of the patronal system is described by Saller in terms of the types of goods and services emperors, aristocrats, governors, and provincial elites had at their disposal and how they went about dispensing them. If it was a question, for example, of the emperor's patronal role, then we should know how many and what types of offices and goods he had to hand out. Since the Romans chose not to devise a rationalized method of selecting officials we should know the exact process by which candidates came to the emperor's attention. Individual aristocrats, in their turn, had favors to bestow as did the governors of provinces and the commanders of legions. In this patrimonial system proximity, or at least access to the emperor (or his friends), was a crucial element for successful recommendation.

Saller's decision to confine himself within the strict limits of subject matter and methodology made it inappropriate for him to pursue at any length some of the more interesting side aspects or implications of his study. It would be useful, for instance, to know what he thinks made the Roman patronal system so readily acceptable to the extraordinarily varied peoples who made up the Roman empire or why, in comparable situations, the Chinese or Ottomans devised radically different administrative systems. Why was it that the Romans themselves were content with the relatively primitive commendatory system they inherited from the republic even though they were aware of the existence of alternatives? On a somewhat different issue, the smooth functioning of Saller's patronage network makes one wonder what happened to the intrigue, violence, ambition, and greed so familiar from most of the narrative histories of the empire. Although often enough disagreeing with Fergus Millar's views, Saller's own findings tend to confirm Millar's image of the emperors as passive reactors. But, as Saller himself notes, his methodology is more effective in describing the permanent aspects of a society than in dealing with change or, it should be added, the exercise of power.

None of these criticisms, however, invalidates or detracts from Saller's superb portrayal of the functioning of patronage in the early empire. There are plenty of accounts that focus on the traditional aspects of imperial history. Until now, at least, we have lacked a comprehensive, analytical account of this central structure of Roman imperial society.

D. BRENDAN NAGLE
University of Southern California

TONY HONORÉ. *Emperors and Lawyers*. London: Gerald Duckworth; distributed by St. Martin's Press, New York. 1981. Pp. xv, 190. £35.00.

The theme of this book is much narrower than its bold title might suggest. Tony Honoré's concern is with one of the major sources for Roman private law during its late classical and early postclassical periods. During the Severan era (A.D. 193–235), the "free" law-finding of the Roman jurists enjoyed an Indian summer of astonishing brilliance. But after 235, during the long years of near anarchy that preceded Diocletian's restoration (A.D. 284–305), the imperial chancellery largely replaced the jurists as the source of law and of legal innovation.

The *Codex* of Justinian (enacted in A.D. 529) preserves nearly twenty-five hundred rescripts on private law from the period A.D. 193–305; of these, more than four-fifths are exactly dated. In his first two chapters, Honoré discusses the origin, development, and operation of the "rescript system," where-

by private citizens or officials could obtain from the emperor authoritative rulings on the content of private law. Honoré argues that the rescripts of this period, though they purport to give the emperor's own words, were in fact written by his secretary *a libellis*, and by him alone; in this period the secretary was normally a jurist. Further, Honoré believes that the rescripts are transmitted more or less intact by the *Codex*.

These controversial claims set the stage for the second two chapters, the book's real center of gravity. In these chapters, Honoré attempts to deduce, from the surviving rescripts, the tenures of the secretaries *a libellis* from A.D. 193–305. Honoré develops a number of "criteria for recognising changes of style," above all "length of rescript, logical structure, syntax, vocabulary and conceptual framework" (pp. 50–53). By contrast, "attitudes to problems of substantive law are, as a matter of principle, disregarded" (p. 50). These criteria are then employed to identify at least nineteen distinct secretaries *a libellis*, some of whom Honoré attempts to name, and the results are summarized in a table (pp. 144–46). The number nineteen is a minimum; Honoré has no way to bridge the awkward gaps, like those from A.D. 246–54 and 260–83, when his evidence simply gives out.

This is the sort of book that will appeal to those who like this sort of book. Not all readers will respond to proofs that rely mainly on the selective presentation of stylistic elements. Part of the reason is that Honoré, for all the vigor of his own style, cannot reproduce the process of discovery that led him to his initial scheme of secretaries; instead, he must develop that scheme, and the evidence to support it, in final form, with initial doubts and alternatives either muted or suppressed.

That said, it must also be acknowledged that Honoré's case is not implausible or even unpersuasive. His exhaustive presentation of stylistic evidence rarely exceeds the limit of fair advocacy, and he continually emphasizes the need for combining criteria, "until conviction grows that the borderline between tenures is in focus" (p. 52). This reader's only major complaint is that Honoré's use of statistical evidence (especially as to average length of rescript) is poor; he gives no standard deviations or tests for significance.

Emperors and Lawyers is less successful than Honoré's earlier *Tribonian* (1978), mainly because the story that it tells is not very exciting. With "problems of substantive law" banished from consideration, the exercise of restoring a list of secretaries becomes highly academic, one that is perhaps better pursued in an *Einzelchrift* than in an expensive (though beautifully produced) book. Indeed, it is never quite clear what sort of audience this book is intended for; the early chapters are too general for

a specialist, and the later ones too detailed for any but a specialist. It is mainly the specialist who will draw profit from the book.

BRUCE W. FRIER
University of Michigan,
Ann Arbor

J. R. MARTINDALE. *The Prosopography of the Later Roman Empire*. Volume 2, A.D. 395–527. New York: Cambridge University Press. 1980. Pp. xli, 1342. \$140.00.

It was Sir Lewis Namier who more than anyone else pioneered the application of prosopographical methods to historical research. But for him the details of individual careers and family connections were not assembled for their own sake, for sheer antiquarian interest. On the contrary, he saw prosopography as a means to an end, his monumental volumes on the eighteenth-century House of Commons being, as he put it himself, an attempt "to analyse it from the angle of purpose" (*Structure of Politics at the Accession of George III* [p. xi]). It is worth noting that he began with a *problem*—what he called "the imperial problem during the American Revolution"—and only came to prosopography by chance, as it were, as a tool to help him get to grips with the problem.

Of course, Namier had to do most of his own prosopographical fact-grubbing, often in the dust-filled attics of old country houses. But, to ease the path of future generations of historians he initiated the grand scheme of a *History of Parliament*, a project that is now well under way.

The prosopographies of the ancient world, such as J. Kirchner's *Prosopographia Attica*, the *Prosopographia Imperii Romani*, and the *Prosopography of the Later Roman Empire* (PLRE), should also serve the same purpose. The ravages of time have at least reduced the records of ancient times to more manageable proportions—though the vagueness and incompleteness of many of the records create problems for the prosopographer of antiquity that do not exist to quite the same degree for his modern counterpart. But, if a prosopography is to be seen as a historical reference tool, as a means to an end rather than as an end in itself, this requires that the work have the qualities of accuracy, consistency, objectivity (or, at least, as much as is humanly possible), convenience and ease of reference, and completeness.

PLRE, initiated by the late Professor A. H. M. Jones of Cambridge, is planned to cover the whole period from 260 to 640. Volume 1, spanning the years 260 to 395, appeared in 1971. The present volume starts with the latter date, which marks the death of Theodosius I and the final division of the

Roman Empire into two, and goes down to the accession of Justinian in 527. PLRE 2 covers the same geographical area as the first volume, that is, the whole of the fourth-century Roman empire, even after parts of the empire had become barbarian kingdoms. This was probably the right decision, as it is in the interest of continuity. Some of the other criteria for the inclusion or omission of information are less easily understood, however—or, for that matter, justified. One is the rather curious policy adopted in regard to the emperors themselves: "The careers of emperors are dealt with only down to the time when they began to rule as Augusti or when they became Caesars, being resumed where necessary if they abdicated or were overthrown" (p. vii). As a result, Theodosius II, for example, is dispatched in half a page—in an entry that includes not a single mention of the *Theodosian Code*. There is not even a list of emperors to be found here, though lists of many other officeholders are collected in an appendix. The nearest thing to an emperor-list are genealogies of some of the imperial families (p. 1308 and following)—with all dates studiously avoided.

A glance at the *Fasti*, or lists of office-bearers, is alarming. One cannot help noticing how very few of these men are recorded as having more than one name. This is not because they did not *have* more than one name; most Romans at any rate would certainly have had at least two: a traditional-type *nomen* (for example, Anicius, Petronius, Domitius) as well as a *cognomen* (for example, Maximus, Paulinus, Julianus); but in the fifth century (and later) the *nomen* is frequently unknown. And yet it is the *nomen*—though not in isolation—that gives us our best clue to ancestry and family relationships (see my *Senatorial Aristocracy in the Later Roman Empire* [p. 103 and following]). The chief reason for this serious gap in our fifth-century sources is the comparative paucity in this turbulent period of the detailed epigraphical evidence in which the fourth century is so rich.

PLRE is so anxious to avoid any duplication of material to be found in the French-based *Prosopographie chrétienne* that clerics are largely absent from its pages. Yet, duplication between the volumes of PLRE itself is indulged in with gay abandon. Individuals whose careers straddle 395 (with the exception, needless to say, of emperors) are to be found in both volumes 1 and 2, and those whose lives were interrupted by the year 527 will, we learn (p. vi), not only appear in the present volume but will "of course, also be recorded in PLRE III" (why "of course," we may well be excused for wondering).

There may be a good reason for this internal duplication after all. PLRE 1 had A. H. M. Jones's name emblazoned on the cover together with those

of two coauthors. On the title page of the present volume his name is conspicuous by its absence. Yet, not only was he the moving force behind the whole project, publicizing the idea and attracting financial support, but he also laid down the main guidelines for the compilations as well as doing some of them himself. It would indeed have done more honor to the *Prosopography* than to his reputation to have published this volume under his name as before. The resulting confusion for library users of the future—in their frenzied attempts to track down the elusive second volume of Jones's *PLRE*—can well be imagined. Here, then, we may have the secret reason for the internal duplications: each volume is evidently designed to be self-contained in case it is isolated from the rest of the set!

M. T. W. ARNHEIM
University of Witwatersrand

TIMOTHY D. BARNES. *The New Empire of Diocletian and Constantine*. Cambridge: Harvard University Press. 1982. Pp. xix, 305. \$35.00.

In recent years, there has been a flurry of scholarly studies of the late Roman empire. Apart from personal predilections, the reasons for this widespread interest in "the triumph of barbarism and religion" present an interesting problem in the sociology of knowledge. Timothy Barnes is an able and experienced campaigner in the late imperial field. His *New Empire of Diocletian and Constantine* (1982) is "conceived as a companion volume to *Constantine and Eusebius* (1981) to argue in detail dates and facts which are there assumed," for example, imperial titulature, the families of the emperors, imperial residences and journeys; lists of consuls, praetorian prefects, urban prefects at Rome, and assorted vicars and governors; administrative divisions and the imperial census. Since sources are amply cited and scholarly controversies are examined in detail, the book is doubly useful as a reference work and as a survey of current debates, though Barnes is no slave of donnish consensus. On intricate problems of evidence and interpretation, he has consulted a number of sages, but he knows his own mind and challenges them all on the date of Constantine's birth and many other historical details. He is convinced that the census of Luke 2: 1–2 was "empire-wide," despite Mommsen's still-convincing rejection of the tale. Barnes is scathing in his criticism of inaccuracies in *Prosopography of the Later Roman Empire*, and he provides some valuable comments on names in *Acta Martyrum* and on documents relating to Donatists. All students of history would do well to heed his warning: "A genuine document is not necessarily a truthful one" (p. 239). Like Diocletian and Constantine, Barnes is often authori-

tarian, and some of his judgments on ancient evidence and modern scholars will receive correctives in the coming years. Yet, critics should recall Gibbon's words: "It is difficult to arrange with order and perspicuity the various transactions of the age of Constantine; and so much was I displeased with the first essay that I committed to the flames above fifty sheets." Thanks to Barnes, writers on the Constantinian era now have a few more facts or probabilities to deal with, and this is no small accomplishment when one remembers the terrible silences that make up most of ancient history.

THOMAS W. AFRICA
State University of New York,
Binghamton

MEDIEVAL

ANNE LLEWELLYN BARSTOW. *Married Priests and the Reforming Papacy: The Eleventh-Century Debates*. (Texts and Studies in Religion, number 12.) New York: Edwin Mellen Press. 1982. Pp. xi, 275. \$29.95.

The subject of married priests has both an intrinsic importance for the study of the Middle Ages, and a relevance to current Christian concern. This book conveniently brings together most of what is known of the history of married priests in the eleventh and twelfth centuries. Less successfully, it also attempts an overview of the history of married priests from the origin of the church. Most controversially, the book presents a brief (see especially p. 195) in favor of clerical marriage. The origin of the book in a dissertation is revealed in a number of ways. Special weight is given to an extensive citation of documents, while reference to modern scholarship in languages other than English is sometimes thin. Unfortunately, a good deal of carelessness is evident in a number of misquoted Latin texts and typographical or spelling errors that occur throughout the book: on page xi the author thanks the typists for having "provided beautiful typing, with narry [sic] a complaint . . ." The index is particularly carelessly composed.

The treatment of the period before 1050 is the least satisfactory part of the book. In describing this as a period in which the church tolerated clerical marriage, an inadequate sense of the mixture of resignation before the inevitable, toleration, and sporadic attempts to attack this state of affairs, is conveyed. Especially odd is the almost complete absence of reference to the patristic and Carolingian attempts to form what the eleventh century called the regular clergy. This reviewer is not a biblical scholar, but, in the light of the prolonged controver-

sies over such passages as Matthew 19:10–12, one might suggest that a simple reference to “the silence of the early church concerning the sexuality of Jesus” (p. 19) is as inadequate as, in the light of the prestige of celibacy in a good number of second- and third-century writings, it is to say “that the church did not recommend celibacy for its ministers.” The description (p. 192) of celibate freedom for pastoral service as a less ancient rationale than the need for priestly sexual purity should have confronted the patristic and early medieval reading of such Pauline passages as 1 Corinthians 7:32 and following.

Chapter 2 considers “The Gregorian Attack Against Married Priests.” The force and single-mindedness of Gregory VII’s attack on clerical marriage is clearly portrayed, and the Anglo-Norman area is taken as a case study of the implementation of the Gregorian decrees. The argument, which at times is hypothetical and polemical, is that by the time of Calixtus II the papacy had committed itself to the proposition (p. 101) “Once a man is ordained that ordination takes precedence over any other vow.” The next two chapters, on the defense of clerical marriage, are the most important in the book. The suggestions for dating the Norman Anonymous make good sense. A final chapter opens many perspectives by relating the success achieved by the Gregorians to more general social changes.

GLENN W. OLSEN
University of Utah

T. H. LLOYD. *Alien Merchants in England in the High Middle Ages*. New York: St. Martin’s Press or Harvester Press, Sussex, England. 1982. Pp. 253. \$28.00.

Those who have admired the earlier writings of T. H. Lloyd, especially his magisterial *English Wool Trade in the Middle Ages* (1977), will be pleased to find that this new book meets his previously established high standards of historical scholarship. My initial reaction on perusing this book, however, was one of disappointment in finding that it covers only a small slice of the Middle Ages: the late thirteenth and early fourteenth centuries, with the lion’s share devoted to the third of a century from the *Carta Mercatoria* of 1303 to the eve of the Hundred Years’ War, 1336. Although I still wish that he had extended it another fourteen years, to the commencement of that other classic study on this theme, Alice Beardwood’s *Alien Merchants in England, 1350 to 1377* (1931), Lloyd’s narrow time-frame is fully justified by his approach, which would have required possibly another eight hundred pages, and many more years of research, to cover the medieval

era up to 1500. In the first place, Lloyd has justly based his study to a large extent on the customs accounts, enrolled and particular, which became so voluminous after the *Carta Mercatoria* and the 1303 New Custom on alien trade (and so much more so after the Cloth Custom and Tunnage and Poundage subsidies of 1347–50). In the second place, Lloyd’s thesis is that the role of alien merchants must be examined within the general context of English medieval trade and that study “cannot be divorced from that of international politics” (p. 61). Thus, after general introductory chapters on “The King, the Community of the Realm, and the Aliens” (merchant law, legal status) and on “Alien Trade, 1303–36: A Long View,” he provides separate chapters on France, the Low Countries, the North (Germany and Scandinavia), and the South (Iberia and Italy) to make detailed examinations of England’s economic and political relations with these regions and their merchants. Perhaps because of this format, there is surprisingly almost no mention of the Jews: certainly Edward I (if not Lloyd) considered them “aliens” in expelling them in 1290.

One of Lloyd’s most interesting observations is that the Italians, certainly the most important aliens in England, were among “the least successful in obtaining perpetual grants of corporate privileges” (p. 166), in striking contrast to the Germans, who were even more divided politically at home but were successfully united as merchants in England. The resolution of this paradox he finds in the Italians’ precocious business techniques and commercial organization, which obviated the need for hanse-type corporate institutions in England—but surely only in the short run. In his concluding chapter, challenging Postan’s negative view of alien contributions to English medieval economic development, Lloyd argues that they were indeed very important, both in facilitating royal finance (as Michael Prestwich had already argued) and in conducting so much foreign trade, when England’s still slender resources permitted its merchants to expand trade just to Flanders and Gascony. This is a most important study, with two valuable appendixes on alien trade, 1303–36.

JOHN H. MUNRO
University of Toronto

JOHN GILLINGHAM. *The Wars of the Roses: Peace and Conflict in Fifteenth-Century England*. London: Weidenfeld and Nicolson. 1981. Pp. xv, 274. £12.50.

Considering how much has been written about the Wars of the Roses, it is distinctly strange that until recently no work has appeared devoted specifically to the military history and logistics of the period.

This is one of two books that have recently filled the gap.

John Gillingham interestingly compares the nature of warfare in fifteenth-century England with that of contemporary Europe. European warfare was a highly technical matter of fortifications, long drawn-out sieges, attrition, devastation, and even at times "scorched earth" policies. Though the English were quite aware of these methods and had in fact used them in Normandy and Gascony, methods of warfare in England were completely different. Up-to-date fortification had passed fifteenth-century England by—witness the evidence of town gates. The West Gate of Canterbury built in the fourteenth century, solidly constructed with gun ports, was obviously a significant military structure. The fifteenth-century South Gate at King's Lynn with wide doorways and large windows was no more than a barrier constructed for municipal purposes such as the collection of tolls. Gillingham might well have taken the trouble to date these buildings more precisely. His argument (with which I entirely agree) would have been strengthened if he had also pointed out that London just did not consider it worthwhile to rebuild its ruinous walls and if he had made considerably more of the civilian, defenseless styles of domestic architecture. The Wars of the Roses, unlike contemporary Continental wars, were decided by short campaigns and, on the whole, short, decisive battles. In thirty-two years (1455–1487) actual campaigning time was something in excess of only *one* year. Consequently there was little destruction and devastation.

In a large section of the book Gillingham describes the campaigns against their political background. The description of the political background is generally adequate though occasionally there are surprising omissions. For example, it is somewhat misleading to analyze the political atmosphere of the 1450s without mentioning acts of resumption, which were one of the major issues in the House of Commons. Gillingham wisely resists the temptation to reconstruct the course of the various battles in detail, pointing out that the sources are completely inadequate for the purpose. The nature of most of the contemporary sources used is adequately described, though one would like to see a more detailed criticism of the Burgundian chronicler, Jehan de Waurin, whose works contain many details not to be found in the, on the whole, meager English narratives of the period. As the detailed interpretation of many incidents is confused and conflicting, readers would benefit greatly from detailed footnote references, which, unfortunately, are nonexistent. This is really a major blemish in a quite valuable book.

Finally Gillingham concludes that the wars were in no way due to deep structural tensions in English

society. In an age of intensely personal politics, they were due to the shortcomings of individuals, in particular those of Henry VI, the earl of Warwick, and Richard III, and they in no way affected the fundamental nature of English society and politics. This is a sensible book, which readers at many different levels will find interesting and informative.

J. R. LANDER

University of Western Ontario

NORMAN MACDOUGALL. *James III: A Political Study*. Edinburgh: John Donald; distributed by Humanities Press, Atlantic Highlands, N.J. 1982. Pp. 338. \$38.00.

Scottish politics and government in the fifteenth century are only recently explored fields, and Norman Macdougall has made a significant contribution with this political biography of King James III (1460–1488). The initial chapters focus on the reign of his father and then on James's own minority in the 1460s, which witnessed the rise and fall of the careerist Boyd family and the emergence of the first Campbell earl of Argyll to a position of pre-eminence among the Scots magnates and state officers. Subsequent chapters, the core of the book, deal closely with James's actions and policies in the 1470s and 1480s, structured around the rebellions of 1482–83 and 1488. The final chapter engagingly surveys the later literature about the king—as sketchy as it is misleading or inaccurate—developed above all by the sixteenth-century writers Robert Lindsay of Pitscottie and George Buchanan.

Reformation writers saw James III as a sort of cross between a tyrant and a *roi fainéant*. Macdougall comes close to agreeing with the former, although not the latter, appellation. James is consistently treated as perhaps the most unpleasant of all the Stewart monarchs: duplicitous, willful, arrogant, and mean spirited. His efforts to make Scotland a major European power were almost pathetically wrong-headed, and his one statesmanlike policy, peace with England, served largely to alienate the southern Scots nobility dependent on border warfare. Debasement of the coinage, a harsh and one-sided administration of the law, and other misuses of patronage and prerogative, provoked—in Macdougall's view, justifiably so—the rebellions of the 1480s. The judgment is unrelenting: James was a king who both courted, and deserved, disaster.

The judgment, on the evidence, seems unfairly one-sided. Political recklessness and dangerous ambition were widely shared commodities. James's abilities to thwart the Boyds and to surmount the revolt of 1482–83 led by his uncles Atholl and Buchan and his own brother Albany—the Clarence of Scotland, as Macdougall himself remarks (p.

212)—testify to this fact as well as to the resources, and the resourcefulness, of the Stewart monarchy. Similarly, with respect to the final rebellion of 1488, while the provocation of the Humes seems clear enough, the reasons for the defections of Argyll and the future James IV are simply too obscure to permit assigning the blame exclusively to the king. Nor is James's defeat at Sachieburn dramatic retribution for his reign; his death, as Macdougall has carefully reconstructed it, was nearly as fortuitous as that of his father at Roxburgh twenty-eight years earlier. In short, the case against James III is to a large extent plausible but too narrowly partial to carry full conviction.

The style of the book is as severe as its interpretation. There are no maps or genealogical charts or other aids for the nonspecialist or the general reader. Even the specialist must maintain constant vigilance to get through the book undaunted. Nonetheless, this is a major installment in the current reconstruction of fifteenth-century Scottish history as a whole.

MICHAEL ALTSCHUL
Case Western Reserve University

LUKE E. DEMAIRE. *Doctor Bernard de Gordon: Professor and Practitioner*. (Studies and Texts, number 51.) Toronto: Pontifical Institute of Medieval Studies. 1980. Pp. xii, 236. \$18.00.

Bernard de Gordon, of Provençal rather than Scotch origin, spent his entire career in the teaching and practice of medicine at Montpellier at the time the medical studium of that city was at the height of its prestige. A host of well-known persons—for example, Ramon Lull, Guy de Chauliac, Guillaume de Nogaret, Francesco Petrarca, Arnold of Villanova, and Henry de Mondeville—spent some time there during Bernard's lifetime. Although Bernard became a standard medical authority shortly after his death, he has never been the subject of a full-length, comprehensive study.

Luke E. Demaitre has attempted a synthesis of all that is known at present about the life and works of Bernard. About his life there is precious little. From Demaitre's portrait, Bernard emerges as a dedicated teacher, well read in medical literature, experienced in medical practice, and more at home in the practical than the theoretical side of medicine, who wrote for his students rather than his peers, and who spurned the high fees and noble associations of his colleagues to devote himself to the teaching of medicine. He expressed contempt for the venality, incompetence, and irresponsibility of his colleagues and for this reason was not popular with them.

For Bernard's writings, the material is much fuller and more reliable. Building on the prelimi-

nary bibliographical work of Karl Sudhoff and numerous recent special studies, and adding much from his own knowledge of the editions and manuscripts of Bernard's writings, Demaitre has placed Bernard's works in chronological order and shown their interrelationships as well as distinguished the authentic from the spurious. On the basis of the canon thus established, Demaitre has studied the "Mainsprings of Bernard's Teaching: Tradition, Reason, Experience, Nature" (chap. 4) and his views on the personal and professional ethics of a physician (chap. 5). The study concludes with superb bibliographies of manuscripts, printed editions of primary works, and secondary studies.

Demaitre has many of the strengths and shortcomings of his subject. His treatment is thorough, clear, and brief, much better on the technical side than the theoretical. There is a disconcerting amount of naïveté, especially in the first chapter, and much effort is wasted seeking answers for which the requisite sources do not exist. There are also some editorial perversities, such as giving a reference (p. 24) for a citation of Alfred Sarashel's *De motu cordis* to "TK 96 and 1020; Thorndike, *Magic*, 2:195; and Sarton, *Introduction*, 2:561" rather than to the edition of Alfred's text by Clemens Baeumker, *BGPM* 23 (Münster, i. W., 1923).

Such shortcomings aside, this book is an indispensable basis for any further study of Bernard de Gordon, and it illuminates somewhat more fully the state of the medical profession in Latin Europe at its best around the year 1300.

RICHARD C. DALES
University of Southern California

JAN ROGOZIŃSKI. *Power, Caste, and Law: Social Conflict in Fourteenth-Century Montpellier*. Foreword by JOSEPH R. STRAYER. Cambridge, Mass.: Medieval Academy of America. 1982. Pp. xxii, 200.

Struggles over taxes at Montpellier are described by Jan Rogoziński as taking place not between the masses and the oligarchy but instead within the latter body. He also believes that the role of the monarchy in this and other battles has been caricatured by liberal and Marxian historians. The crown sincerely strove for social peace, he thinks, and judicial arbitration helped achieve it. Roman law did not favor monarchy as much as it protected men's rights. This view of Roman law is surely partly correct, but the author then goes on to assert that "Renaissance princes found it more difficult than their 'feudal' predecessors to levy taxes and raise armies precisely because written law and . . . precedent . . . guaranteed personal and provincial privileges." Yet, based on a population hardly greater or richer than that of 1325, Charles VIII had

larger armies on longer service and bigger fortifications than Philip the Fair.

A prosopographer, Rogoziński is good at career histories, but he sometimes stretches his material too far. A law professor knighted by the king of Majorca, Seguer was the son of an apothecary and the father of a son who claimed nobility. Seguer's appointment to the law faculty had been "forced through by the lord of the town only with . . . pressure from the papal court." Claiming that the chair was his to dispose of, the bishop had objected. That says something about institutions, but how about Seguer? Charged with having instigated the murder of a merchant, the *jurista* was tried and condemned to exile, the loss of privileges, and a heavy fine. Was the trial really "marked by favoritism"? Well, Seguer was soon back in circulation.

Prosopography, of course, is not new. In the thirties, a refugee from East Europe's revolutions named Nicola Ottakar taught in Italy that class analysis was insufficient to illuminate the history of the medieval city. What he, as did Lewis Namier, a similar refugee busy elsewhere, wanted instead was to examine each party, family, and individual. Like our author, this historian attacked the weaknesses of liberal and Marxian class analysis advanced by Rodolico, Salvemini, and others. To choose one, however, must one exclude the other? Individuals, especially leaders, play many roles and become partly classless, but classes and groups possess real identities. The fact that their cohesion eventually fails proves only that man is transitory.

Implicit in Rogoziński's view is the notion that, for a movement to be really popular, the "poor" cannot be led by the "rich." A wealthy lawyer and knight could not have led a "group that seriously threatened the established order." But who knows what the *jurista* thought about his "group"? Plugging "reform," he was perhaps willing to risk a little effervescence. Nor do all leaders of popular movements come up from the cellar. Marx was sometimes strapped for money, but he had been well educated, married a gentlewoman, and was served by a female family retainer for about forty years. And what about the industrialist Engels and his relationships with his workers, the women, for example, of the Burns family? Solid burghers these, and the latter surely a gentleman! This example—and many more: Florence's knightly Giano della Bella, and so forth—shows that those speaking for the "poor" are rarely poor themselves. Unable to direct themselves, the "humble" often entrust leadership to those prepared by means of wealth.

In short, this is a book pointing to the right direction but given to overstating its case.

JOHN HINE MUNDY
Columbia University

BRIAN PATRICK MCGUIRE. *The Cistercians in Denmark: Their Attitudes, Roles, and Functions in Medieval Society*. (Cistercian Studies Series, number 35.) Kalamazoo: Cistercian Publications. 1982. Pp. xiv, 421.

Brian Patrick McGuire frankly admits that his goal was not to be original but rather to familiarize a non-Scandinavian audience with the activities of the Cistercians in a fringe country of Europe. As far as a member of that audience can judge, he has succeeded. Although the sources are far from abundant, McGuire has not merely recounted the dreary details of property acquisitions and the monks' quarrels with bishops but has also probed the Cistercians' changing perceptions of themselves and their role in society. McGuire's basic story is a familiar one. Starting as spiritual revolutionaries in the twelfth century, the Danish Cistercians quickly adapted to local conditions and became successful, but conservative rural entrepreneurs.

The Cistercians established ten monasteries for men and three convents for women between 1144 and 1215 in medieval Denmark, a kingdom that included parts of modern Sweden and Germany. Archbishop Eskil of Lund (1137–1177), their chief patron, summoned the Cistercians to strengthen the Danish church's links with the universal church rather than to engage in mission work or to clear new land. The friars replaced the monks in the spiritual vanguard in the thirteenth century, but the Cistercians preserved their religious and economic ties to the aristocracy. They were hurt by the weakening of the Danish monarchy, the decline in papal prestige, and the agricultural crisis of the later Middle Ages, but McGuire concludes that the monasteries were still in good shape at the eve of the Reformation. The maps and lists of Danish kings, bishops, and abbots are useful reference tools.

I was disturbed, however, by some of the things that I could judge. McGuire has confused the papal schism between Innocent II and Anacletus with Frederick Barbarossa's quarrel with Alexander III (p. 6). This is incredible because St. Bernard was a leading figure in the schism of 1130 and because the mistreatment of Archbishop Eskil was the subject of the confrontation at Besançon between Barbarossa and the future Alexander III.

Mistakes like this make me suspect that someone was napping at Kalamazoo. Places that are known by a variety of names in different languages always pose a problem, but it makes little sense to use the Danish form for places situated outside Denmark, for example, Rygen for Rügen in East Germany; and it is sloppy to refer to a bishop of Kamien on page 82 and a diocese of Cammin on page 231. Single quotation marks are employed where standard English usage requires double quotation marks. Inexplicably, British spelling is used in a

book written by an American and published in the United States. Some of the writing is more appropriate for an undergraduate than a mature scholar: for instance, "Gervais describes how he had chatted up another notary. . . ." (p. 116). Finally, the following sentence speaks for itself: "Under this queen and her male predecessors was ruled and misruled the medieval Denmark which provided the setting for the Cistercians this book treats" (p. 7). Such blunders mar a good book.

JOHN B. FREED
Illinois State University

JAN T. HALLENBECK. *Pavia and Rome: The Lombard Monarchy and the Papacy in the Eighth Century*. (Transactions of the American Philosophical Society, number 72, part 4, 1982.) Philadelphia: The Society. 1982. Pp. 186. \$18.00.

For some fifteen years Jan T. Hallenbeck has been actively exploring in journal articles the relationships between the Lombard monarchy and the papacy in the eighth century. This monograph is an attempt to examine the eighth-century relationship between the Lombard crown and Rome, with particular emphasis on the period from the Lombard-Byzantine peace in 680 to the conquest of the Lombard Kingdom by the Franks in 774. Even though the ground covered by this study has been well plowed by several generations of scholars, Hallenbeck contends that the older works "lack a clear and persistent focus upon the dealings between the monarchy and the papacy" (p. 1). His approach is admittedly narrow, concentrating exclusively on political dealings between Pavia and Rome; his methodology is straightforward, supplying a chronological narrative supplemented by occasional analysis; his objective is avowedly revisionist, implying a need for reappraisals of Lombard hostility toward Rome.

The general thrust of Hallenbeck's argument is that Lombard kings, beginning with Liutprand, worked consistently for the creation of a Kingdom of Italy, which would have incorporated northern Italy, the coastal regions from Ravenna to Rome, and the duchies of Spoleto and Benevento. Rome and its territory would have been left in peace so long as the popes did not interfere with Lombard designs. Only on rare occasions, principally in the actions of King Aistulf in 753, did Lombard kings plan to conquer Roman territory. According to Hallenbeck, the numerous Lombard incursions into papal territory during the eighth century were normally motivated by a Lombard desire to force the popes to cease actions disruptive to the realization of the Lombard Kingdom of Italy. The eighth-century tension between the Lombards and the papacy was due, therefore, to the fact that the

Lombard vision of the Italian future was often unacceptable to Rome and, conversely, the Lombard kings would not tolerate the papal vision of Italy wherein the Lombards were restricted to the north.

This view is the result of a minute description and analysis of the relations between Pavia and Rome, with careful attention to the settlement between King Liutprand and Pope Gregory II in 728, the agreement of Terni in 742, the conquest of the Exarchate of Ravenna by King Aistulf in 751, the initial Frankish involvement in Italy from 753 to 756, the *modus vivendi* between King Desiderius and Pope Paul I in 760, the confusion in Rome following the death of Paul I in 767, the accommodation between King Desiderius and Pope Stephen III in 770-71, and the events leading to renewed Frankish intervention in Italy beginning in 773. In each episode political dealings between Pavia and Rome are analyzed in the context of Lombard designs to construct their Kingdom of Italy, papal attempts to alter the Lombard plan, and Lombard pressure designed to force the popes to cease their disruptive actions. Although there is little new in this chronological narrative, Hallenbeck clearly articulates his argument at each step. The six maps included in the study are excellent, but the bibliography is somewhat brief.

In my judgment, however, the book has some serious flaws. Among them is a tendency to rely on the older works of Thomas Hodgkin and Ludo Moritz Hartmann rather than subjecting the admittedly meager source material to careful scrutiny. Also, there is an insistence that in the eighth century Lombard monarchs were attempting to construct a Kingdom of Italy. Such an insistence would hardly provoke debate, but it is never completely clear why this entity would not include Roman territory. Further, there is no attempt to analyze Lombard charters or church foundations to discover what the Lombard kings said about themselves, nor is there reference to works on that topic by Herwig Wolfram, Carlrichard Brühl, and Hermann Fröhlich. Pointing to the lack of Lombard charter analysis, however, may be criticizing Hallenbeck for a book he did not intend to write. Finally, there seems to be a somewhat uneven use of source materials. The papal letters to Venetia and Grado in the 730s are accepted as genuine, although there has been considerable scholarly discussion regarding their authenticity. Papal intervention in the Frankish-Bavarian dispute in 743 is assumed, even though the only record of it is in the *Annales Mettenses priores*. The origins of two of Pope Stephen III's letters to the Franks (*Codex Carolinus* 45 and 48) are explained in a manner that is contrary to long-held scholarly opinions, and Hallenbeck's analysis of the letters is not particularly persuasive.

The relationships between Pavia and Rome in the eighth century were complex ones, and Jan Hallenbeck is to be commended for his careful chronological reconstruction. The flaws in the book, however, keep it from being a superior study.

DAVID S. SEFTON

Ohio Northern University

A. IA. GUREVICH. *Problemy srednevekovoi narodnoi kul'tury* [Problems of Medieval Popular Culture]. Moscow: Iskustvo. 1981. Pp. 357. 1 r. 70 k.

In this book the Soviet medievalist A. Ia. Gurevich (known primarily as a student of early Scandinavian history from his former books and contributions to the periodical *Srednie veka*) has tackled for the second time the problems of medieval culture in Western Europe. This book follows his earlier *Kategorii srednevekovoi kul'tury* (1972), but, if the earlier book studied this subject *in toto*, the book under review has limited its attention to only one layer of this culture, namely the culture of the mass of the people, the *illiterati*, very little studied *per se* so far. Furthermore, the author has restricted his investigation to the "dark period" of the sixth to the thirteenth centuries, more characteristic of the Middle Ages than the later period verging on the decline of its culture ("the autumn period," according to J. Huizinga).

In his task Gurevich has faced one basic difficulty: how does one get at the culture of the *illiterati* by the study of sources produced by the *literati* (for the *illiterati* themselves have hardly registered their feelings and ideas)? He has solved his task by attaching himself to those products of the *literati* that had been conceived for the sake of reaching the mass of the people in the middle Latin literature from the sermons of Caesarius of Arles (died in 542) to the *Dialogus miraculorum* of Caesarius of Heisterbach (Cistercian monk who died about 1240).

Chapter 1 of the book gives the general picture of this literature in its basic varieties, taken up by the author one by one in the following chapters. It is important to point out that Gurevich has consciously refrained from tracing in his presentation the historical development of these manifestations of the medieval culture. To see more clearly "its internal system which remained hardly mobile during these seven or eight centuries, repeatedly reproducing its basic features" (p. 14), he treated this material synchronistically, a novel approach to this particular subject.

Thus, one by one he has examined the lives of saints, the penitentials, the tales of the world beyond the grave (chapter entitled "The Divine Comedy" prior to Dante), and, finally, the theological literature for the use of the people (with special attention given to the *Elucidarium* of Honorius Augusto-

dunensis, first half of the twelfth century, and *Historiae memorabiles* of Rudolf von Schlettstadt, thirteenth century). All along there is ample reference to the documentation provided by plastic arts. There are also numerous pertinent examples drawn from a very wide range of up-to-date literature that is overwhelmingly West European. Among the Russian quotations one must note special appreciation of the prerevolutionary historian L. P. Karsavin (*Osnovy srednevekovoi religioznosti v XII-XIII vekakh, preimushchestvenno v Italii* [1915]) as well as of the late Soviet philologist M. M. Bakhtin.

In his inquiry of the sources mentioned above, Gurevich has been looking especially for the different stages of a phenomenon that he calls "the paradox of the medieval culture," namely the symbiosis in the same consciousness of both its learned and popular aspects (pp. 13 and 72). The study of the lives of saints has shown him a strong and constant hagiographical tendency to treat the saint primarily as a benevolent sorcerer, thus for the sake of vulgarization transforming the very bases of the Christian religion. The penitentials are studied by him against the background of the parish. The preoccupation of the confessors with black magic and its ritual transfers us, as he says, into "a world usually concealed by official Christianity" (p. 153), a stubbornly stable world of immemorial ancestry, which, in the minds of the faithful, coexists with the teaching of the church.

The eschatological aspect of this problem has led Gurevich to the study of ideas, spread among the mass of the people, on life beyond the grave, and, in this connection, of the representation of the other world in ecclesiastical art and, especially, in the tales of visits to that world or visions thereof. This study leads the author again to stressing the difference between the official theological eschatology and the perception of the mass of the people, which transferred the judgment of the sinner and the salvation of the righteous from the end of time to the moment immediately following death (individualization of the Last Judgment). As Gurevich put it, "biography seems to triumph over history" (p. 237). In this way the common medieval man brings together the present moment and eternity, and easily moves from the world of the living into that of the dead and vice versa; he also endows the human soul with material features. The church, however, did not perceive this as anything heretical, accepting this dichotomy as a requirement of reality.

Finally, as the theological *summae* were beyond the intellectual capacity not only of *vulgus profanum*, but of a considerable part of the lower clergy, treatises were written with a simplified explanation of the basic theological tenets. Such was the very popular *Elucidarium* by Honorius Augustodunensis studied in the book in greater detail. This study led Gurevich to stress a considerable rift between theo-

logical thought as developed in the twelfth to fourteenth centuries and the ideas prevailing in the religious mind of the mass of the people, "a complex of notions formed toward the end of the eleventh century to the beginning of the twelfth century and moreover simplified and dogmatized as reflected in Honorius Augustodunensis' dialogue" (p. 270). Two different levels of medieval religiousness!

The final chapter is devoted to sketching the relationship between these two levels of culture: the "top" and the "bottom"—the medieval grotesque, in Gurevich's formulation. In his development the author dwells on the ambivalent character of the medieval popular mind, which easily combined terror and laughter, as shown especially in the treatment of the devil. He also stresses the tendency to express spirituality in concrete, sensual, and material terms. Gurevich saw in "this continuous movement from contrasting to blending and from blending to contrasting the field of action for grotesque thinking" (p. 321). In the presentation of his ideas the author leans strongly, though at times differing from them, on the ideas of M. M. Bakhtin.

Although not yet arriving at a complete picture of medieval popular culture (in his afterword he himself calls for a further study of it) the author has sketched enough of its basic elements to make his book a valuable contribution.

MARC SZEFTEL
University of Washington,
Seattle

MODERN EUROPE

WINFRIED BAUMGART. *Imperialism: The Idea and Reality of British and French Colonial Expansion, 1880–1914*. Translated by the author, with the assistance of BEN V. MAST. Foreword by HENRI BRUNSCHWIG. New York: Oxford University Press. 1982. Pp. xi, 239. \$34.95.

H. JOHN FIELD. *Toward a Programme of Imperial Life: The British Empire at the Turn of the Century*. (Contributions in Comparative Colonial Studies, number 9.) Westport, Conn.: Greenwood. 1982. Pp. xiv, 256. \$29.95.

Winfried Baumgart's *Imperialism*, a work on "the idea and reality" of British and French colonial activities from 1880 to 1914, is most welcome. Baumgart, Professor of Contemporary History at the University of Mainz, presents the leading theories of imperialism and demonstrates how many of the facts of the period contradict them. With perhaps pardonable hyperbole, he suggests that "there are more people who have written on imperialism than people who have acted on it or fought for it"

(p. 8). He has brought together a good number of these studies for discussion, and the effect is on the whole illuminating.

Baumgart has divided theories of imperialism into four major groups. One framed along political and historical lines includes such diverse motives as religion and antislavery, exploration, the new technology, and great-power rivalry among the impulses for expansion. A second category of "national- and social-psychological" theories argues that nationalism, navalism, and Social Darwinism are the essential clues. The third grouping consists of the economic theories of Hobson and Lenin that have dominated much of the literature on empire. Finally, there is what he calls the "social-economic theory" of social imperialism.

Baumgart's purpose is to show that none of these explanations, considered exclusively, is adequate. To understand the phenomenon of imperialism as a whole, all must be brought into play. In a particular case, however, only "a small bundle of factors"—whose composition will vary from instance to instance—was probably decisive. Only those unfamiliar with writings in the field will wonder why Baumgart insists at such length and with such force upon so reasonable a proposition. But specialists will recognize that monocausal explanation, sometimes tangentially qualified, characterizes all too many works in the field.

Baumgart has his preferences among the theories he examines. The politicohistorical explanation, supplemented by the national- and social-psychological one—particularly, the power-political motive and the impulse of great-power rivalry—seems to him to best explain expansion during this period. In this, he follows the lines of his own work on German imperialism and the studies of Henri Brunschwig on French imperialism. Baumgart's principal target is the economic explanation, and he effectively displays the difficulties to be found in the Leninist theory. While prepared to acknowledge that the economic motive often played a secondary role, he stresses that at times it was entirely absent. Equally an object of his attack is the theory of social imperialism, at least when it is employed by Hans-Ulrich Wehler as a general explanation of the phenomenon of European imperialism. Baumgart also somewhat tendentiously takes exception to Wehler's use of this theory in the latter's well-known analysis of Bismarckian policy, although at the same time he speaks approvingly of studies of British social imperialism.

There are a number of points to which a reviewer might take exception. For example, Baumgart misconstrues certain of the arguments concerning an "informal" imperialism, and ignores others; moreover, he gives too little notice to Schumpeter's sociological theory. Still, overall, his work should exercise a benign influence.

H. John Field's *Toward a Programme of Imperial Life* attempts what Baumgart would describe as a "national- and social-psychological" explanation of colonial expansion. In a historiographical chapter, Field examines the principal scholarly and popular efforts to explain British imperialism at the turn of the century. The purpose of this interesting, though not entirely reliable, account is to suggest that no one has as yet attempted "in any systematic fashion" to present an "attitudinal" study of how the late Victorians saw empire building as a "manly" reaction against the *fin-de-siècle* decadence. Field tells us that these Englishmen regarded imperialism as a duty undertaken to strengthen both individual and national "character." This was an important enterprise since they saw all good things (family stability, national efficiency, and even survival) as ultimately dependent on its success.

Such a theme is worth exploring, and, in different guises, scholars have not neglected it, although they have not pursued it as methodically as they might have done. Field's method, however, far from being systematic, takes the form of a rambling, impressionistic group of essays. In two of these the author presents the views of a number of English and foreign observers, all without any proper biographical introduction, whose ideas are often only peripherally related to his subject. Two further chapters, one-third of the book's contents, on G. W. Stevens, a once well-known newspaperman who worked for the *Daily Mail* during the 1890s, constitute the book's chief claim to originality. Field sees the racism and popular Social Darwinism of Stevens's articles as relevant to his theme because the journalist employed analogies between national and individual characteristics. This is neither especially revealing nor persuasive.

Field on occasion displays a thoughtful and perceptive scholarship, but he does not have his study under control. There is a good deal of material—I would guess the residue of essays on a variety of imperial subjects—yet not enough on the subject he has chosen to discuss. It is an omnium-gatherum. The concluding chapter is marred by pretentious and at times indecipherable jargon. All this is unfortunate, since there is evidence of a capacity for something better.

BERNARD SEMMEL
State University of New York,
Stony Brook

DIETRICH EICHHOLTZ and KURT GOSSWEILER, editors.
Faschismus Forschung: Positionen, Probleme, Polemik.
Berlin: Akademie. 1980. Pp. 459. 28 M.

The articles in this volume from East Berlin purport to provide insight into international scholarship on

fascism in the last decade or two. As has been the custom in the Communist bloc, "fascism" is defined throughout the book as the global political expression of what East European historians like to call the "monopoly bourgeoisie" (*Monopolbourgeoisie*) of capitalist-oriented societies. Hence individual historical figures, such as Benito Mussolini or Adolf Hitler, are said to be neither the creators nor the movers of "fascist" ideologies, but simply the instruments of "monopoly capitalism." Their personal roles are minimized, whereas the corporate ones of the upper or ruling classes ("owners of the means of production"), proverbially portrayed as the foes of the working class, are emphasized. Since the working class and its value system are assumed as a positive standard, "fascism" in this volume is largely defined antithetically, that is, in terms of classic interpretations by Lenin, Georgi Dimitroff, or (in this East German case) by Walter Ulbricht.

It is possible to divide the articles in this book into three separate (but sometimes overlapping) groups. The first of these represents attempts at outright propaganda, allegedly based on orthodox Marxist theory and with little or no effort to utilize empirically validated facts. Thus Elfriede Lewerenz deals with the definition of the "imperialistic" character of fascism by the Communist International (1922–1935). Ernst Gottschling writes about "The Fascist State: The German Example," in which references to "fascism" in the West German Federal Republic become well-nigh obligatory. Of the same ilk is Manfred Weissbecker's contribution, "Fascism at Present: Bases—Manifestations—Activities—Organizations." By far the most pedestrian example of this genre is Vera Wrona's lecture, "Marxist-Leninist Fascism-Critique: A Necessary Prerequisite for the Antifascist-Democratic Mutation."

A second group of essays attempts to accommodate critically the most recent historiography on national socialism and fascism in the West without mere recourse to pre-established Marxist wisdom but by trying, instead, to discern and sometimes to appreciate different shades and degrees in theory, methodology, and the specific ideological settings within which the Western authors wrote. In this appraisal, well-known historians of the West German political left (Reinhard Kühnl, Dirk Stegmann, Reinhard Opitz) fare rather better than internationally and solidly established "liberal-bourgeois" historians like the American Henry A. Turner, Jr., or even the moderately "revisionist" writer Timothy W. Mason from England. The best of these papers is that by Gerhard Lozek and Rolf Richter, entitled "Toward a Discussion of Prevalent Bourgeois Theories of Fascism," not least because it shows an intimate knowledge of Western historiography as well as a sense of understanding why the Westerners may have written in the mode they did. Articles such

as this could establish a common meeting ground for any future discourse on currently contentious issues with Eastern colleagues.

The third group of papers, best liked by this reviewer, dispels the stereotype still largely upheld in the West that East European historians, because they are beholden to their brand of Marxism, cannot generate new historical insights at all. Klaus Drobisch's piece, "About Terror and its Institutions in Nazi Germany," provides a comprehensive treatment of the systematic establishment of oppression in Nazi Germany as an instrument of governance and, in so doing, for once shifts the focus somewhat away from SS and Gestapo in the direction of the Nazi party and its affiliate institutions. In this way the reader is effectively reminded that terror was indeed a total system built into every part of the vast and complex Nazi machine. On another note, Eberhard Hackethal seeks to define fascism in Latin America. While his party-line intention may seem obvious, he does point to common manifestations of strongman dictatorships that are often swept under the carpet in the West because the Latin American subcontinent is indispensable to it. The article by Heinz Malorny on "Nietzsche and German Fascism" is equally suggestive. It may well be that the author simplifies Nietzsche as a precursor of national socialism. But he also rightly points out that Nazism was not generated in a vacuum, and he reminds the student of history how potentially lethal some of Nietzsche's ideas, born of his own peculiar sentiment of cultural pessimism, actually were.

On balance, half of the essays in this volume may be used to good advantage by Western readers, if only to receive fresh impetus for thought. Despite the unavoidable ideological lecturing, the general impression is a positive one, namely, that the East German scholars are trying to come out of their shell, are seeking better working contacts with Western colleagues, and can demonstrate a basic willingness to come to grips with these colleagues' own interpretations. As their invective is dwindling, their vocabulary is obviously becoming more temperate, not to say objective. From a Western hemispheric position, it is hoped that this trend will continue.

MICHAEL H. KATER
York University

HANNES JÓNSSON, *Friends in Conflict: The Anglo-Icelandic Cod Wars and the Law of the Sea*. Hamden, Conn.: Archon or C. Hurst, London. 1982. Pp. xi, 240. \$34.00.

The extent to which the geographical circumstances of a nation influence its political behavior has long been a subject for academic debate. Hardly debatable, however, is the influence of geographical

conditions on the small wars between the United Kingdom and Iceland over fishing rights off the Icelandic coast—rights regarded as important by each of the adversaries. But whereas the alleged rights were primarily regarded as matters of principle in the United Kingdom, they were seen in Iceland as crucial to survival.

This account of the four British-Icelandic conflicts (1952–76) over Iceland's fisheries limits, including the three Cod Wars, has been written by an Icelandic scholar-diplomat who accompanied the prime minister of Iceland to the 1973 meeting in London at which a solution to the third fisheries conflict was negotiated. As might be expected, Hannes Jónsson sees the conflict from an Icelandic point of view. But he puts the Anglo-Icelandic issue into the broader context of the evolving law of the sea; in this instance, the crucial issue was the extension of national sovereignty beyond the three-mile limit that was widely accepted before World War II. He identifies advocates of extended exclusive jurisdiction as the "Progressive School" of thought on the law of the sea and the West European adherents to a traditional three-mile limit as the "Colonial School." The propriety of these designations is the only significant doubt that I have concerning the substance of this book.

The greater part of *Friends in Conflict* relates the incidents involved in the four disputes. Events followed a repeated pattern. Four times between 1952 and 1975 Iceland extended the limits of its maritime jurisdiction: from 3 to 4 miles (1952), 4 to 12 miles (1958), 12 to 50 miles (1972), and 50 to 200 miles (1975). Following three of these extensions the British government, rejecting Iceland's unilateral action, sent contingents of the Royal Navy to protect British trawlers in the waters in which they had been accustomed to fish.

The conflict was "friendly"—neither party resorting to lethal violence. British ships incapacitated Iceland Coast Guard vessels by ramming—fifty-five times during the last Cod War (1975–76). But the Icelandic Coast Guard during the same period cut trawls off forty-six British and nine West German vessels fishing inside the 200-mile limit. A ban was placed on landing Icelandic fish in British ports, but the fish caught by British trawlers in "Icelandic" waters during these "comic opera" wars must have been the most expensive ever landed anywhere in the history of fisheries.

In the end the British and their West European allies capitulated to the trend toward extended jurisdiction. Ironically, the British government had been moving in this direction while simultaneously attempting to maintain traditional limits in the North Atlantic.

Friends in Conflict is most significant, however, in the broader context of the changing international

legal order. It is a case history; an illustrative episode in the revision of the law of the sea. The sections dealing with the development of international law—especially the United Nations law-making process (pp. 23–30) are exceptionally clear and concise. This useful book contains a number of statistical appendices and an extensive bibliography, and is adequately indexed.

LYNTON K. CALDWELL
Indiana University,
Bloomington

BARRETT L. BEER. *Rebellion and Riot: Popular Disorder in England during the Reign of Edward VI*. Kent, Ohio: Kent State University Press. 1982. Pp. x, 259. \$18.00.

During the six and a half years when the dukes of Somerset and Northumberland successively governed England in the name of the boy-king, Edward VI, more of those who were governed probably committed more acts of riot and rebellion than at any time before the Civil War. What caused the “commotions” that so terrified contemporary chroniclers and court preachers? In some cases rapidly dictated changes in religion helped spark socioeconomic distress into outright resistance to government policy; epidemic disease, inflation, and repressive landlords variously explain the sources of some of the distress. But some rebels were financially comfortable yeomen loyal to the ruling regime; neither rents nor Reformation defined their cause.

By trying to place the rebels and their causes “at the center of the stage,” Barrett L. Beer has proclaimed himself free of the blinkered perspective of historians “sympathetic” to the governing *élite* (p. ix). Approving, introductory references to the work of Marc Bloch, Le Roy Ladurie, and some recent investigations of the German Peasants’ War seem to advertise what follows as social history of an advanced or fashionable type. In fact, in this and his other aims—“to examine all forms of popular disorder from 1547 to 1553”; “to view popular discontent as a national threat to law and order”; “to show the interrelationship between local communities and the central government” (p. ix)—Beer falls far short of the mark.

The author has rightly emphasized the extent of popular unrest and the problem of explaining its multiple origins, but without a taxonomy or typology of popular reaction he cannot adequately describe either the character or the significance of the disturbances in question. In essays of forty-three pages on the Western Rebellion, fifty-seven on Kett’s movement, and twenty-three on “all other parts of the realm” (excluding London, which merits a brief reprint of an article of 1972 on defensive

preparations there), Beer does little more than outline what happened in 1549. (He surveys the “struggle for stability,” 1549–53, in another twenty-eight pages.) There is fresh material here—Beer cites manuscripts in nine English provincial record offices and four London repositories—but with few exceptions the evidence cited is not of a type capable of yielding new information about the motives of the participants. The result is a curiously updated kind of antiquarian history; the narrator cannot fit the rebels’ actions into meaningful contexts.

In a brilliant, original essay of 1979 (*Past and Present*, 84 [1979]: 36–59), Diarmaid MacCulloch suggested just such a properly local context for the East Anglian stir, showing how misleading it was to think that Kett had promoted a “rebellion” there. Beer cites parts of MacCulloch’s story, but passes silently over his conclusions, including the startling and persuasive one that in some areas subject to the fold-course system of pasturage tenants, not lords, sought enclosures. Having committed himself to a mode of historical writing the limitations of which MacCulloch had already exposed, Beer apparently chose to ignore the fresh perspective MacCulloch’s contextual example provided for the whole history of riot and rebellion in Tudor England.

DALE HOAK
College of William and Mary

PETER LAKE. *Moderate Puritans and the Elizabethan Church*. New York: Cambridge University Press. 1982. Pp. viii, 357. \$49.50.

This monograph by Peter Lake has most of the virtues of the breed: it is a thoroughly researched and clearly written presentation of evidence relating to a chronologically limited and analytically specific—if, in this case, generically indeterminate—subject. Although this study of “moderate Puritans” in the Elizabethan church does not include the most important of that inchoate lot, William Perkins, that it should succeed in spite of such an arbitrary omission (Lake’s explanation is not satisfactory) is a tribute to the craftsmanship with which Lake probes the careers, social environment, and thought of Puritan notables at Cambridge University. An examination of the “ideological and social practice” of Edward Dering, Lawrence Chaderton, Thomas Cartwright, and William Whitaker reveals to the author “a certain spectrum of opinion” stretching from a presbyterian “left” to an “entirely conformable . . . right.” In other words, the object of Lake’s analytical effort is to construct yet another definition of “Puritanism.” The major flaw in his method is made obvious from the outset: Lake declares he wants a “nominalist” proof of a confessed “need” for a “concept of ‘puritanism’ as a coherent, logically

and emotionally related set of opinions and attitudes" (p. 11).

What follows is necessarily unsatisfactory as definition, but there are many pages of intelligent and informative description. The section on "Chaderton's puritanism," which opens the argument, also characterizes it. The key to Chaderton's Puritanism, we are assured, was his view of "the godly commonwealth rested firmly on scripture" (p. 52). Yet in the next paragraph we are reminded that its realization was in practice tempered by moral ambiguities engendered by academic and personal ambition. Just how Chaderton's scripturalism differentiated a "Puritan" position in the Elizabethan church we never really learn. Did not all Protestants believe that to some similarly ill-defined extent godliness rested on biblical foundations in family and commonwealth?

Subsequently, Lake is bold enough to argue that Patrick Collinson's view of the disastrous fall of Grindal and the related split between the hard-line Field group in London and the academic and political Puritans is incorrect; that throughout Whitgift's primacy the classical movement remained firmly joined to the "mainstream of moderate puritan thought" (p. 73). The conformists failed, he asserts, "to drive a wedge between the radicals and the moderates of the puritan movement" (p. 75). Indeed, Whitgift emerges as the rather heroic architect of Calvinist consensus made explicit in the Lambeth Articles.

Lake's evidence for this and most of his other assumptions and arguments is engagingly unconvincing. It is engaging because he cites with honesty the ambiguous and contradictory sources from Cambridge college life. In the complex, bitter fight over Whitaker's rule at St. John's, the issues of Calvinism were secondary to the jealousy felt by senior fellows passed over in the advancement of Whitaker. Indeed, the major part of Lake's investigation uncovers petty, comical, futile aspects of university politics, which will be depressingly familiar to all readers who are academically employed. The real conclusion of his study may be that academic Puritanism was as fortuitous and nebulous as all other forms of Protestant enthusiasm. It remains difficult to write the internal history of so transient, inconsistent, and recurrent a phenomenon as English Puritanism. The sources for exploring Puritan thought are splendid; the historical conceptualization of that thought remains inadequate.

C. H. GEORGE
Northern Illinois University

PETER HOLMES. *Resistance and Compromise: The Political Thought of the Elizabethan Catholics*. (Cambridge Studies in the History and Theory of Politics.) New

York: Cambridge University Press. 1982. Pp. viii, 279. \$39.50.

This is the first book comprehensively covering English Catholic political thought under Elizabeth. It is a major contribution. Peter Holmes has done his research thoroughly, writes well, and makes some very good original points. To my mind, the best and most original part is part 2, "Religious Resistance," which describes the complex and ambivalent ways in which Catholics dealt with the conflict between their obligations to confess the faith and avoid participation in heresy and the state's demands for religious conformity.

The ambivalence with which Catholics thought about resistance to demands for religious obedience reflected the conflict between the need to maintain their identity under persecution and the need to compromise with the larger society to avoid extinction. The conflicting demands of their situation also affected their view of the main directly political question that they faced—namely, should they support or oppose efforts to restore Catholicism by force?

Holmes believes there were no clear or lasting divisions among the Elizabethan Catholics based on political ideology. Instead, he sees the Catholic body as a whole oscillating between resistance and nonresistance. Holmes sees Catholic thought as basically nonresistant for most of Elizabeth's reign, with resistant interludes during 1569–73 and 1584–96, when the restoration of Catholicism by force seemed a serious possibility. He reaches this conclusion largely by arguing that William Allen and Robert Persons, widely regarded as the leaders of militant Catholicism, did not decisively opt for political resistance until 1584, and that Persons (Allen died in 1594) retreated from political resistance after the mid-1590s.

I find this picture more convincing before 1596 than after. This is partly because I would interpret the writings of Allen and Persons in light of their actions, and as Holmes at times implies (for example, pp. 63–64 and 215), they were more consistently resistant in action than in print. Allen and Persons were more concerned with restoring Catholicism in England than with building up a consistent body of political theory, and often tailored their propaganda to the demands of a particular audience or a particular situation.

I think Holmes underrates the differences in political theory after 1596 between Persons and his opponents in the ostentatiously nonresistant Appellant party. He underrates the radicalism of Persons's *Memorial for the Reformation of England*; much of the book's program, as he says, is not peculiarly Catholic, but sixteenth-century people were made into religious radicals not so much by what theology they

accepted as by how important they thought a particular form of religious practice was. The *Memorial's* program to establish religion as the dominant concern in English life could have been adapted to Catholic or Puritan purposes, but it was anathema to conservatives who valued the English political hierarchy for its own sake.

Holmes's last chapter, which is the center of his argument for the nonresistant Persons of 1596–1603, is based largely on the *Temperate Ward-word* to Sir Francis Hastings and pays little attention to the anti-Appellant works of 1601–1602. Even so, he spends about 45 percent of the chapter on a section entitled “vestiges of resistance theory”—surely an odd distribution for someone arguing that Persons was at this time a nonresistant thinker.

As is natural for a reviewer, I have focused on what seem to me to be Holmes's most debatable points. My disagreements cover a relatively small proportion of the book, and they do not detract from my opinion that this is a most valuable work, which anyone interested in the subject should read.

ARNOLD PRITCHARD
New Haven, Connecticut

DAVID S. KATZ. *Philo-Semitism and the Readmission of the Jews to England, 1603–1655*. (Oxford Historical Monographs.) New York: Clarendon Press of Oxford University Press. 1982. Pp. viii, 286. \$39.95.

About twelve months after Peter Stuyvesant grudgingly allowed a boatload of Jewish refugees to settle in Nieuw Amsterdam, the question of a formal invitation to Jews to settle in London was agitating the highest circles of the English government. The Jews had been expelled from England in 1290, but by the middle of the seventeenth century a clandestine group of them was living in the English capital. Acting on his own initiative, Manasseh ben Israel (1604–57), an eminent Dutch rabbi, having deduced on the basis of Scripture that the establishment of a Jewish community in England would hasten the advent of the Messiah, sought a formal invitation for Jewish immigrants from Oliver Cromwell. The Lord Protector, conscious of the advantages to English trade of the presence of the Jews, convened a conference of lawyers, merchants, and divines in December 1655 to consider the petition. Opinion was divided—the merchants being particularly hostile—but the lawyers insisted that there was no legal barrier against Jews living in England. Fortified by that opinion and anxious to avoid a negative statement from the conference, Cromwell let it disperse without issuing an opinion regarding an invitation; but the government informed the crypto-Jews that if they demeaned themselves peaceably they could rely on protection. Under

these conditions they continued to live under both the Commonwealth and the Restoration. In that informal manner—with immigrating Jews treated as any other inhabitants under the common law and not as the *corpus separatum* proposed by Manasseh ben Israel—the resettlement of Jews in England was accomplished. As David S. Katz felicitously concludes, “Cromwell and Charles II realized that the Jews as a nation could never be admitted through the front door,” but “they were anxious to go round the back themselves and let them in through the entrance reserved for tradesmen.”

This story has been a prime target for the attention of students of the history of Jews in England. One thinks of the pioneering researches of Lucien Wolf and the later studies of Cecil Roth. Katz's monograph is more detailed, though its focus is narrower. Legal and mercantile concerns enter only incidentally. Mosaic law, the glorification of Hebrew, millenarianism, and the lost ten tribes had become subjects of intense discussion under the impact of the Reformation; and these are the subjects closely examined in this book.

Katz carries out his task with all the signs of meticulous scholarship. Having combed the archives, he presents a great deal of detailed information on the various manifestations of interest in the Jews that appeared in the decisive half-century. Occasionally the detailed examination of the trees makes the reader forget the contours of the woods, but this does not diminish one's respect for the soundness of his scholarship and the productiveness of his research.

S. D. TEMKIN
State University of New York,
Albany

AUSTIN WOOLRYCH. *Commonwealth to Protectorate*. New York: Clarendon Press of Oxford University Press. 1982. Pp. xii, 446. \$49.95.

Austin Woolrych, professor of history at the University of Lancaster, already the author of numerous articles and books on the English Revolution, has made another important contribution to our understanding of the period with the publication of his new work. Basically a political narrative, *Commonwealth to Protectorate* focuses on the brief period surrounding Barebone's Parliament of 1653, and yet it also manages to shed light on the entire era. In this respect the book achieves the high standard for political history attained by two other recent works, David Underdown's *Pride's Purge* and Blair Worden's *The Rump Parliament*. Although minor disagreement with the latter is expressed in an early chapter (mainly over interpreting Cromwell's moti-

vation for dissolving the Rump), the three authors agree on the fundamental dynamic of these years. They all see the last decade of the revolution as reflecting the tension between radical Puritanism and moderate constitutionalism, a tension never fully resolved until the Stuarts were restored.

Generally considered as a manifestation of radical Puritanism running wild, Barebone's Parliament is traditionally presented as a fit topic for denigration and ridicule. It is one of Woolrych's purposes to offer a more judicious appraisal. Essential to his approach is a recognition of Cromwell's need to balance the demands of the army with a commitment to legality and parliamentary government. With this in mind, a call for such a nominated assembly as Barebone's Parliament is seen by Woolrych as a temporary solution to a political dilemma, rather than as an attempt to join hands with the Fifth Monarchists in order to achieve an instant millennium. In fact, Woolrych is able to destroy the popular myth that the parliament was controlled by religious zealots. He demonstrates quite convincingly how moderates dominated every strictly political decision, from reform of the law to the handling of the Leveller, John Lilburne. This much-maligned body, frequently identified with the Tower of Babel, managed to pass thirty statutes within the five months that it was allotted. Indeed, we learn how the belittling of their efforts stems, in part, from the snobbism directed by contemporaries toward a parliament whose members were considered social inferiors. But here too Woolrych demonstrates how exaggerated were popular misconceptions, as virtually four-fifths of the MPs actually belonged to the gentry class.

Even though Woolrych is able to minimize the religious character of Barebone's Parliament, it is undoubtedly true that ecclesiastical disputes served to destroy its existence as a constituted body. Religion remained the most explosive topic throughout this entire era, which explains why earlier John Pym and his successors either managed to suspend discussion of the subject or sought to form as broad a consensus as possible. Because true believers in considerable numbers were present in Barebone's Parliament it was inevitable that the religious issue would eventually arise and that once it did all progress in civil matters would come to a halt. What is amazing is that a religious impasse did not emerge at the very outset and that Barebone's Parliament accomplished so much. Austin Woolrych has placed this complicated and controversial subject in an appropriate historical perspective. By so doing, he has added immeasurably to English seventeenth-century scholarship.

LAWRENCE KAPLAN
City College
City University of New York

REED BROWNING. *Political and Constitutional Ideas of the Court Whigs*. Baton Rouge: Louisiana State University Press. 1982. Pp. xi, 281. \$27.50.

Reed Browning makes a substantial contribution to the growing literature on Court Whig political thought in England after the Glorious Revolution. Recent studies have redressed a balance too long tilted toward Opposition Whig (or Patriot) propaganda because these writings were a principal means through which radical reform ideas were transmitted from the mid-seventeenth to the late eighteenth century. Most work on Court Whiggism, however, has concentrated on the years between 1689 and 1720, when the Whigs first had to struggle with the problems of being responsible for the government of England rather than its permanent critics. It has also emphasized single themes, such as changing interpretations of the revolution. Browning's aim is rather to elucidate court political and constitutional ideas in the age of Walpole and the Pelhams, when the revolution was no longer a subject for serious debate, and to stress the diversity of Court Whig defenses of government within a framework of several often inconsistent assumptions and approaches.

The core of his book lies in five essays on the political apologetics (to call them political philosophies would be too kind) of five rather different Court Whigs: the arch-courtier, John, Lord Hervey; the aggressive controversialist, Bishop Benjamin Hoadley; the more benign moralist, Archbishop Thomas Herring; the interesting if muddle-headed historian, Samuel Squire; and the great legist and statesman, Philip Yorke, Earl of Hardwicke, the only superior mind among them. Another chapter analyzes convincingly and succinctly the major themes developed in the large body of other pro-government pamphleteering, and the whole is set in a framework of the transition from a Catonian ideal model of intransigent political virtue to a Ciceronian model of moderation directed by the idea of overarching natural law. This last theme, although a currently fashionable mode for interpreting movements in Augustan thought, seems rather contrived, but the rest of the exposition of ideas is usually convincing.

The Patriots had an edge in style and intellectual capacity and certainly carried forward ideologies whose later consequences would have appalled most of their advocates; but the Court Whigs, for all their platitudinous tedium (even Browning is sometimes exasperated by them), had more practical sense. They had also, he makes clear, more intellectual quality than they have usually been credited with. They had to defend government in general, not merely the Walpolean-Pelhamite regime, so they had to, and did, address the central problem of

political thought—how to reconcile liberty and order—and did so in a variety of ways. They were obsessed, as was nearly everyone else in eighteenth-century England, with the notion of balance (although what was balanced, and how, varied from author to author) and, as government apologists, were more concerned with “popular” threats to “true liberty” and stability than with the dangers from governmental power. One of the more surprising aspects of their arguments is how little they relied on Locke and how much more they relied on Hooker and even neo-Harringtonianism. The Court Whigs emerge as more intellectually respectable and creative than they have been thought to be and as contributors to the subsequent development of English political thought as significant as their Patriot opponents. Browning’s book is a thoughtful, comprehensive, thoroughly researched study that is essential to all future work on English political thought.

ROBERT A. SMITH
Emory University

J. C. D. CLARK. *The Dynamics of Change: The Crisis of the 1750s and English Party Systems*. (Cambridge Studies in the History and Theory of Politics.) New York: Cambridge University Press. 1982. Pp. xiii, 615. \$69.50.

This book seeks to present a radical reinterpretation of eighteenth-century politics by identifying a “deep and silent shift in the nature of political conflict” that occurred as a result of the “crisis of 1754–57” (p. 447). In these years the Whig and Tory parties, which had dominated English politics since the reign of William III, were destroyed in the course of a bitter struggle between Newcastle, Pitt, and Fox for control of the Whig old corps. This, in turn, leads to the assertion that “what was importantly different in the 1760s was not a new, designing monarch, nor the absence of a Prince of Wales . . . but the absence of a Whig-Tory alignment in the old sense” (p. 448).

J. C. D. Clark argues that historians have failed to recognize this crucial development because they have been trapped in a persisting Whig view of the eighteenth century that has obscured the continuity and importance of parties under the first two Georges. Unfortunately, Clark offers no satisfactory definition of party, and his case is weakened by the differing and sometimes contradictory ways in which he uses the term. Even more serious, because his entire argument depends on it, is his failure to prove that the Whig and Tory parties did indeed dominate politics throughout the first half of the eighteenth century.

Equally troublesome is Clark’s reliance on the primacy of “high politics,” by which he means the tactical maneuvering “among the small group of men at St. James’s and Westminster who really mattered” (p. 3). Other forms of political activity, “argument in the press and the activities of the mob” (p. 3) for example, are dismissed because “the political situation within which these things were important (when, and in so far as, they were important) was created by the politicians” (p. 4). Issues, a term Clark likes to enclose in quotation marks, are significant only as they are manipulated by the leading contenders for power; they “had no necessary impact on parliamentary politics independent of the tactical context within which, and the party vehicles by which, they were given political significance” (p. 211). Particularly scorned is “that nebulous generalisation, ‘public opinion’” (p. 264), for historians, according to Clark, have been unable to prove how public opinion affects elite politics. Instead of investigating ideology, mobs, pamphlets, speeches, patronage, and public opinion, historians should return to the study of “the infinite variety of high political manoeuvre” (p. 314). “It is those tactical complexities,” Clark states, “which are the subject of this study and which reveal other spheres of political action as subordinate” (p. 3).

High politics is to be studied by “pressing further the evolution of narrative technique” (p. 17). Here Clark appears to mean simply a fuller and more carefully wrought narrative than any alternative. Thus the heart of this book is more than four hundred pages on the period from Henry Pelham’s death to the formation of the Newcastle-Pitt ministry. The apparent assumption is that truth will somehow spontaneously emerge from this narrative. But the very act of writing narrative necessarily involves selection and interpretation; and, while Clark is able to correct errors made by other historians, his own version of events contains questionable conclusions on matters as important as the emergence of a Leicester House opposition in the spring of 1755 and the resignation of Newcastle in 1756. Furthermore, his emphasis on elite political maneuver leads to a complexity that only experts in the history of the 1750s can hope to follow. The elimination of almost all background, combined with Clark’s specific refusal to deal with motive or character, results in a text so colorless and so dull that anyone who does not absolutely have to read it will get far.

Those who do persist will discover a book based on truly prodigious research. Clark has searched the papers of virtually every individual even remotely involved in politics during the 1750s. Over one hundred pages of endnotes support this highly detailed account of political infighting. Future his-

torians, whether or not they accept Clark's conclusions, cannot ignore this work.

Even those who are convinced by Clark's arguments will probably be angered by something in this book. Most specialists in the period are explicitly attacked for shoddy scholarship or faulty interpretation. By implication, Clark also condemns the groundbreaking works of E. P. Thompson and John Brewer. Almost all who have written on eighteenth-century England are found guilty of being either "exponents of a 'Whig interpretation of history'" or "their socialist successors" (p. 458). Whole branches of history are censured. Practitioners of interdisciplinary history are described as "usually social historians, unskilled in economics, who seek to annex instead the province of politics" (p. 17). Numerous attacks on constitutional history culminate in Clark's concluding remark on "the effective bankruptcy of the ideology concerning the nature and evolution of political action which is traditionally entrenched in constitutional history" (p. 457). This assault on his peers clearly places the onus on Clark to prove his own case. He has not done so.

JAMES L. MCKELVEY
University of Connecticut

IAN R. CHRISTIE. *Wars and Revolutions: Britain, 1760–1815*. (The New History of England.) Cambridge: Harvard University Press. 1982. Pp. 359. \$22.50.

FRANK O'GORMAN. *The Emergence of the British Two-Party System, 1760–1832*. (Foundations of Modern History.) New York: Holmes and Meier. 1982. Pp. xi, 132. \$10.50.

Each of these books belongs to a series, and the title of each book suggests its scope: Ian R. Christie presents an essentially chronological treatment of a period in British history, and Frank O'Gorman examines a subject identified with the same period, although justice to the subject demands extension of O'Gorman's period seventeen years beyond the terminal date of Christie's. The books necessarily intersect at points along the way. Specialists in political history, both historians share appreciation of the pivotal importance of the formative reign of George III for understanding the modern British political process.

Conforming with the plan of his series, Christie writes what might seem appropriate for the old political history of England series. Except the first and seventh chapters that are economic and social history, the book is political history, broadly considered: given the nature of the period, constitutional, diplomatic, imperial, and military history are included in generous portions. During half the period

covered by Christie the kingdom was engaged in major wars. At home there was chronic political disquiet, seemingly threatening to the peace of the realm. Because the causes of the discontent were only partially dealt with, they presaged troubles for future politicians. As Christie points out at the beginning, the reign saw great change, stress, and danger; "the glow of the golden Victorian age" (p. 1), cast backwards, created an "illusion" of domestic tranquillity for the reign as a whole. Some of the discontent and disturbances did not have political objectives: there was no revolution, and the nation "remained on an even keel" (p. 2). By 1815 a victorious Britain regained the heights of world prestige that the young king knew when he ascended the throne fifty-five years earlier. His was not an inglorious reign.

One of its glories was what O'Gorman calls the emergence of the "duality" (p. ix) of the two-party system. It appeared uncertainly in the last years of the American war, developed jerkily although "irreversibly" (p. x) by 1832, and, to go beyond O'Gorman's book, received classic formulation by Bagehot as the age of Gladstone and Disraeli began. Because the beginnings of the modern two-party system lay in a parliamentary setting and within the political process, in an age that saw the wars and revolutions of which Christie writes, and because O'Gorman's mission is to develop the "duality" theme, he expects his readers to accept that political history does not take place in a vacuum. In that sense Christie's book is context for O'Gorman's study, which understandably treats political history more narrowly. If O'Gorman's focus is national, parliamentary politics, he gives more attention than does Christie to the constituencies. While much research has been devoted to the parties fitfully taking shape in parliament, what was happening in the constituencies and between them and the center needs further investigation. O'Gorman is working on these problems but here can give suggestions, not definitive answers.

On certain key matters attendant on the emergence of the two-party system, such as the nature of parties, the development of the cabinet, appointment and dismissal of ministers, and the overarching problem of the two-party system itself, there is no fundamental disagreement between these authors. Differences of degree or emphasis, of the meaning for party history of certain political crises, and about the stage of maturity attained by 1815 may derive in part from the difference of purpose of each of these books. Christie's is a clinically clear and dispassionate account of things as they were; O'Gorman's is not quite so exclusively an account of things as they were—a goal lies ahead and, toward the end of the book, is in sight. Almost unavoidably,

at stages along the way, he measures implicitly how far he has come and the distance remaining, and so vital years, such as 1782–84, are discussed more tendentiously than in Christie's book. Christie seems more cautious about changes in the character of parties. This is not to say that O'Gorman is incautious, only a bit more assertive and perhaps slightly anticipatory.

Taken together, the books suggest how far toward a new consensus historians have come since Lewis Namier fifty years ago destroyed G. M. Trevelyan's conception of a Georgian two-party system. Subsequent research and debate generated heat and, happily, light; now temperatures have cooled, and the broad outlines of a new version of the development of the two-party system replaces the old, keeping only the name.

CARL B. CONE
University of Kentucky

P. J. CORFIELD. *The Impact of English Towns, 1700–1800*. (Opus.) New York: Oxford University Press. 1982. Pp. vi, 206. Cloth \$23.95, paper \$9.95.

As the first industrialized country, England was the first to urbanize. Yet the eighteenth century has been comparatively neglected by the recent fashion for urban history, so P. J. Corfield's excellent introduction is especially welcome. The series to which the book belongs aims to be both concise and authoritative and to appeal to both the general reader and the academic market. This tall order, with its inevitable compromises, has been adroitly filled. The work is split into two roughly equal halves, the first dealing with different categories of towns and the second with those broad topics—social or political aspects for instance—which all share. Corfield divides her towns into four groups. London naturally has a chapter of its own because of its dominating size and success, with 11 percent of the national population and well over half the total urban population contributing much to the overall achievement of eighteenth-century Britain. She devotes a chapter to the ports and dockyard towns and another to spas and resorts like Bath and Brighton, thus distinguishing the characteristic products of the contemporary expansion in overseas trade, navies, and leisure. This leaves us with the market and manufacturing towns, given rather less space than they deserve, as the industrial towns were a major development of the period and the market towns were still in a numerical majority. In the second half of the book she covers the urban economy, concentrating chiefly on organization. This is the weakest chapter of the book, for much of the material that belongs here was already used elsewhere. Urban demographic surveys are used to explain the phe-

nomenon of mass migration and the problems of birth and death rates. She is at her best when describing urban society. Here, as elsewhere, she covers a wide territory, including religion and cultural life. The variety of forms of government, oligarchy, and radicalism appear in her survey of urban politics. A concluding chapter describes the environment, with its rebuilding and new amenities. A short bibliography of about one hundred items follows.

Corfield is very good at combining many, often amusing illustrations from literary sources with the newer material derived from statistical analysis; here we have the best of the old and of the new schools of socioeconomic history. She strikes an admirable balance between generalization and illustrative detail and avoids vagueness and superficiality. The principal weakness is dictated by the series, which suppresses footnotes to the point where the reader wishing to pursue further some specific, as opposed to general, point is left short of guidance. Are footnotes really so terrifying? The prevailing impression is of urban growth and multifaceted change, a combination of solid enterprise and urbane elegance that here finds an author with matching qualities of mind and pen; a book for anyone interested in eighteenth-century England, and an essential introduction for the urban historian.

ALAN DYER
University College of North Wales

PAUL SAUNDERS. *Edward Jenner: The Cheltenham Years, 1795–1823; Being a Chronicle of the Vaccination Campaign*. Preface by WILLIAM LE FANU. Hanover, N.H.: University Press of New England. 1982. Pp. xviii, 469.

Paul Saunders's book is an exhaustive account of Edward Jenner's later life, concentrating on the period after his first experimental vaccination in 1796 and his subsequent decision to migrate from rural Berkeley to the suddenly fashionable town of Cheltenham. Saunders's primary interest centers on the social milieu within which Jenner promoted his vaccination technique and, in particular, on the social networks that apparently played so great a role in the dissemination of his ideas. In great and sometimes bewildering detail, Saunders supports his opinion that "Jenner rode to victory amid the most licentious society in England and supported by a nobility of rakes and courtesans" (p. xiv). The book is littered with the names of the great aristocratic families, including those of the royal family. Jenner's address book must have looked like a combination of Burke's *Peers* and a contemporary *Who's Who*.

And yet, with all this assistance from the highest

elites, Jenner found that vaccination did not sweep all before it in England. Where the influence of individual aristocrats, or their scions, was strong, immediate headway was possible, as in postrebellion Ireland, the British army and navy, and those parts of the empire where adherents were posted. What seems clear from Jenner's experiences, but unclear from this book, is that either the aristocracy never exerted themselves too much on Jenner's behalf or their patronage was of only limited influence. The former seems more likely. Saunders assumes that the aristocracy supported Jenner purely for humanitarian reasons. There is no suggestion that Jenner might have been used by these society gallants for their own purposes, either to appropriate his personal glory or to gain pleasure from exhibiting a scientific lion (or toy poodle). There is some evidence that Jenner himself was aware of his vulnerability; his reluctance to attend meetings in his honor and to visit London regularly are suggestive.

It is symptomatic that Saunders does not address himself to this type of problem. His approach is essentially that of the antiquarian, more concerned with the minutiae of the lives of the elite, irrespective of their relevance, than with the problems of social and political context that are raised by Jenner's career. We are told that medical opposition was strong because of vested interests, but there is no attempt to examine either the scientific objections to Jenner's system (his opponent, George Pearson, is accused of using contaminated cowpox matter, yet Peter Razzell's claim that Jenner did likewise is ignored) or to analyze this opposition within the framework of pre-existing tensions in the medical community. We are told that the poor and the yeomen farmers, through ignorance, opposed vaccination, but there is no attempt to explain their views in cultural terms. If the idea of using cowpox inoculation as a preventive against smallpox stemmed originally from the rural poor, why did they oppose the process when it was returned to them in scientific and aristocratic clothes?

Thus we are left with a book that raises far more questions than it answers. Even within the confines of Saunders's own limited framework, he does not ask most of the questions that interest medical historians, although as a compilation of stories about the wellborn, the book has much to offer. The late Dr. Viets is quoted as saying that Saunders's work "crosses a new frontier in medical history" (p. xiv); unfortunately, Saunders seems to be unaware both of the location of the current frontier and of the state of contemporary Jenner studies. Edward Jenner still requires a modern biographer.

MICHAEL J. DUREY
Murdoch University

KENNETH BOURNE. *Palmerston: The Early Years, 1784–1841*. New York: Macmillan. 1982. Pp. xiv, 749. \$24.95.

With few exceptions, only Palmerston among the great Victorian statesmen has not received the thorough twentieth-century biographical study that his career warrants. If the first volume of this two-part biography is any indication, this gap will no longer exist. Kenneth Bourne has more than done justice to his subject. He has chosen the most difficult years of Palmerston's life to assess and brought to them fresh perspectives and a unity that will establish this biography as the definitive one for our generation.

These were the formative years. For the longest time Palmerston was of little significance on the national scene. His service in the War Office taught him habits of business that were to serve him well in later life, but it did not enable him to advance in the government. His years at the War Office also demonstrated his capacity to immerse himself in tedium and minutiae. Lesser men, and certainly greater intellects, would have been overwhelmed and would have faded into oblivion as a result. It is a testimony to his real capacity as an administrator and to his faith in himself that Palmerston did not succumb to his long apprenticeship.

Perhaps his survival was a reflection of his enormous appetite for work that was itself part of a larger capacity for living life energetically. These characteristics enabled him to meet the demanding responsibilities of his private life and ultimately to dominate the public arena as well. The style and pattern of decision making were there nearly from the beginning. He shaped up the War Office, grew personally as it expanded in response to war, and continued to develop his own ideas while presiding over its contraction in the postwar years. In the process he effectively transformed it by 1827 into a nineteenth-century department quite different from the bureaucratic nightmare he inherited in 1809. Bourne thus reminds us that the Palmerston who so terrorized the Foreign Office establishment after 1830 had learned his administrative catechism well in the War Office.

The public life of the younger Palmerston, however, is only part of the story. The biography skillfully weaves these features of Palmerston's early career into the general narrative. He favored Catholic Emancipation, the liberalization of the trading laws, and a foreign policy that assured a leading role for Britain in the give and take of East-West relations. In all of these areas he seems to have been influenced by others without becoming their follower. It is curious that only on the matter of parliamentary reform, the central issue of the period that so animated his contemporaries, was he indifferent

if not perhaps even slightly hostile to the views held by most of the liberal Tories.

Finally we come to the Whig ministries of the 1830s when everyone wanted to meddle in his business at the Foreign Office. In that age of declining but still potent amateurism he quickly showed that he was the real professional. The wonder is not that he clashed with them all, though usually only one at a time, but that he managed to maintain and even to strengthen his own position during this decade. Here he was to prove his real strength of character and his capacity for leadership.

The years before 1830 were not, however, merely a prologue to the Foreign Office. Few men in the 1820s would have predicted the extent of his later influence and the central role he was to play on the mid-Victorian stage. Yet in another sense these years were critical for the future. They enabled him to develop a degree of self-confidence that later was to bedevil his opponents and assure his triumphs over them. He was capable of caution when it suited his purposes, but on balance he was, in his public life at least, inclined to pick his ground and to make his stand continuously upon it. Small wonder that more thoughtful men, such as Lord John Russell, were to prove to be no match for him in his later years.

Given Palmerston's public self-assurance, the contrast with his private insecurities is all the more remarkable. It is in the latter area that Bourne, using the personal papers in the Broadlands collection, makes his most telling remarks. Palmerston was capable of both great love and enormous jealousy. His long liaison with Emily, Lady Cowper and subsequently Lady Palmerston, is a testimony to the range of his passions. It is singular that he kept a journal, deciphered here in a clever and informative manner, that recorded his liaisons with her and with numerous others. The energy he expended, even in failures (recorded with equal care), was prodigious. He frequently worked long hours in the day, danced and was amorous into the wee hours of the morning, only to steal home for a few hours of sleep and to repeat the process the next night.

Other personal characteristics show his complexity as a man. Despite his own numerous financial hardships, he took care of his friends and relations, some of whom, such as Sir George Shee and the erstwhile concubine Emma Murray, were to prove to be more trouble than they were worth. Yet even when he had established himself in the 1830s as the principal spokesman on foreign affairs and when his career had elevated him to the first rank of government, Palmerston steadfastly refused to turn his back on those who had been important to his personal life in the early years of his career.

Bourne tells of these changes with grace and style. In leaving no stone unturned he has brought both

the man and the world in which he lived back to life in all their complexity and vivid detail. We are all richer as a result.

CHARLES R. MIDDLETON
University of Colorado,
Boulder

MARTIN PUGH. *The Making of Modern British Politics, 1867-1939*. New York: St. Martin's Press. 1982. Pp. xi, 337. \$25.00.

Martin Pugh has written a text of great usefulness that informs us of the "state of play" in the interpretations of the political story in Britain from 1867 to 1939. It is a work limited in its aims, but highly satisfactory in that it achieves them. Years ago, before the great emphasis on social history, such a study might have appeared as *the* history of Britain, for it tells the central political story of the late Victorian and modern period up to the outbreak of the Second World War. Now political historians are much more modest—perhaps even defensive in some cases, but not here—in their claims, and politics are just one way of presenting the past. Pugh does not claim a special primacy for his account and interpretation; yet he does support a conception of Britain as the country of relentless moderation where politics, despite moments of stress and difficulty, keep returning to a middle position, whatever may be the name of the particular political party in power.

The Making of Modern British Politics is not a monograph; it is rather a sophisticated textbook based occasionally on some primary material but largely drawing on a wide range of books and articles dealing with the period, which have appeared over the last twenty years or so, and assimilating them into a coherent and largely convincing story. (Pugh's guide to further reading is extremely useful.) Here we have the destruction of the Liberal party and its rebirth both in the Labour party—a familiar idea, but very well worked out here—and also in its influence on the Tories, particularly Stanley Baldwin.

Pugh takes part in that continual scholarly pursuit of listening to the heartbeat of the Liberal party in order to record its formal death. He presents a party much less damaged by the Home Rule controversy than is generally supposed and quite hale and hearty up to the First World War. He argues that the leaders of the party rather unnecessarily helped cause its downfall by their disloyalty to its ideas and traditions—Edward Grey through his support of militarism, H. H. Asquith through his mishandling of the franchise issue, particularly his disdain for votes for women, and Asquith's and other Liberal leaders' costly failure to cooperate with organized

labor. 'The Liberals' heart stopped when the coalition government was formed in May 1915. For Pugh these are largely pragmatic rather than ideological matters, although he recognizes both the significance of ideology and popular following. Pugh also has a firm grasp of the relationship of local politics with events in the central government. Although aware of the difficulties of relations between Liberals and Labour in the constituencies, he presents the failures of Liberal leadership as far more important in accounting for the decline of the party.

But, in a sense, it did not matter very much, for in his view the Labour party is, in effect, a continuation of the Liberal party and, perhaps, more appropriately named and structured to deal with the completion of voter democracy. By 1918 franchise reform was virtually complete. A commonsensical democracy had been achieved, and the war had made possible a party based on the working class. "The potential for a governing party based on this working class had existed unrealized for decades, but there are no good grounds for thinking that Labour would have attained this status in the 1920s but for the decisions and blunders of 1914-18" (p. 181).

Pugh is intensely aware of voter behavior and of the working-class Tory voter. As heirs to liberalism, both Labour and Conservatives were able to maintain themselves in the middle and to have turns in power—Labour in 1924, 1929, and finally its great triumph in 1945—beyond the scope of this study but prepared for in the text. The National Government of the 1930s also represents a triumph of moderation. "This curiously unexciting conclusion to a decade [the 1930s] marked by trauma and extremism reflects the skill of both Conservative and Labour leaders in emasculating their respective radicals, isolated on the peripheries; and further, the success of one of the two schools of moderation in outmaneuvering the other on the central ground of British politics" (p. 293).

The first three sections, through the 1920s, where Pugh himself has done original research, are more compelling in their analytical power than the more narrative last section on the period between the wars. There is not too much passion in this text—the triumph of moderation does not make for exciting reading. But Pugh does achieve the difficult task of presenting clearly in short compass both a synthesis of recent scholarship and his own views, placing all students of the period in his debt.

PETER STANSKY
Stanford University

HUGH A. MACDOUGALL. *Racial Myth in English History: Trojans, Teutons, and Anglo-Saxons*. Hanover, N.H.:

University Press of New England or Harvest House, Montreal. 1982. Pp. ix, 146. \$27.50.

Hugh A. MacDougall, author of this slender volume, follows a clearly marked path through the myths by means of which the English people have from the earliest time sought identity and pride. The landmarks are familiar enough and the accompanying travelogue provides few surprises for specialists in English history. For nonspecialists, for students in particular, it provides valuable information not easily obtained or so competently synthesized elsewhere. It could also prove salutary for the uncountable many who still nourish the myths of race in the back rooms of their minds.

The story begins, of course, with the mischievous legacy of Geoffrey of Monmouth, which for centuries satisfied the curiosity of Englishmen about their origins if only, and curiously, in the legend of a band of defeated Trojans, and which provided native Celtic heroes in the persons of Arthur and his knights. Although more the makings of romance and a legendary history readily adapted to political purposes rather than a truly racial myth, the story of Arthurian Britain remained a source of national pride through most of the sixteenth century, and was fiercely defended against skeptics equipped with the new critical techniques of humanist scholarship. As historical study clarified the national past, British antiquity gave way to the more documentable Germanic origins, and the Anglo-Saxon myth was born. It is to this myth, and especially to its flowering in the nineteenth century, that the major portion of the book is devoted. The author follows with clarity and a rigorous economy the uses to which Anglo-Saxonism was put as it settled into the national consciousness to become the inspiration for Victorian jingoism and the rhetoric of empire. Even historians familiar with most of the evidence presented can still be startled by the crassness of statements about race made by such luminaries as Macaulay and Carlyle and by the unscholarly assumptions permitted themselves by such eminent scholars as Froude, Freeman, Seeley, and Green. One might expect Martin Tupper to proclaim, in his ineffable verse, that "The world is a world for the Saxon race," but not so sober a scholar as Goldwin Smith to conclude that "of the physical influences which affect the character and destiny of nations, the most important seems to be that of race." Even before the close of the nineteenth century, however, the increasingly menacing presence of the new Germany had taken a good deal of the wind out of Saxonist sails, and the pseudoscientific perversions of Darwinism and the new physical anthropology were beginning to meet with serious criticism. Yet the Anglo-Saxon myth unquestionably helped create the vision of empire. As MacDougall points out,

its fullest expression "coincided with the highest realization of national aspirations" and "its disintegration was simultaneous with England's decline as a great power."

Despite its very real merits, this book is somewhat disappointing. Perhaps it could not have done more without becoming a very large book; but what is there begs for something more, especially in the chapters dealing with the later periods when racial theories could no longer be taken for granted and when they entered into the life of the time in many and subtle ways. The author is, of course, concerned primarily with Anglo-Saxonism, but any myth of racial superiority implies—and generates—corresponding myths of racial inferiority. The relations of Anglo-Saxonism to the problem of slavery, to the benevolent hypocrisy of the white man's burden, and to Anglo-Irish relations deserve more than the passing attention they are given. So, by the same token, do the factors that eventually undermined the myth of Anglo-Saxon superiority. And the reader may well wish for more about the way the myth was applied in the public forum to the discussion of specific issues.

ARTHUR B. FERGUSON
Duke University

RICHARD ALLEN SOLOWAY. *Birth Control and the Population Question in England, 1877–1930*. Chapel Hill: University of North Carolina Press. 1982. Pp. xix, 418. \$29.00.

Between 1870 and 1930 both the birth rate and the average number of children per marriage in Britain dropped by well over half. This book deals not so much with the underlying reasons for the remarkable decline in the birth rate as with the increasingly heated public debate which accompanied that decline. From 1877, the year of the founding of the Malthusian League, the first birth control organization in the world, to the final disintegration of opposition to the sanctioning of contraception by local health authorities in 1930, birth control was endlessly debated by its chief apologists, notably the Malthusians and Marie Stopes, and its chief opponents, the medical profession and the clergy. The debate addressed both its moral and physical effects on individuals and the effects on the nation and empire of the continuing decline in the birth rate of the well-to-do, a decline that to the Malthusians' great alarm was not accompanied by a similar decline in the birth rate of the poor. Fear of "race degradation" was the Malthusians' stock-in-trade.

This is a study of what was written and said for public consumption, not an effort to examine changes in privately expressed opinions; Richard Allen Soloway cites few letters or diaries. Yet, as he himself suggests, the marked increase in the practice of birth control between 1877 and 1930 probably

had relatively little to do with public discussion of the population question and a great deal to do with economic and personal self-interest, not excluding the self-interest of doctors and clergymen who, while steadfastly opposing birth control for the masses, nevertheless became quite proficient at limiting the size of their own families. In exploring the population controversy Soloway has devoted considerable attention to the arguments of the Malthusian League, which remained the only population control organization in Britain until after the First World War, yet he seems to have found it difficult to assess the League's impact on birth control practice, there being no way to accurately gauge the influence of the three million pieces of literature that the League distributed during its fifty-year history.

The final third of this book deals primarily with Marie Stopes, whose frankness, courage, and sheer truculence ensured her a central role in the last decade of the debate. Soloway also extends our knowledge of the important roles played by Stopes's allies and competitors in the Workers Birth Control Group, the Society for the Provision of Birth Control Clinics, and, indeed, the Women's Section of the Labour party. He is also particularly adept at documenting the ostrich-like refusal of the medical profession, until well into the 1920s, to engage in scientific study of the alleged ill effects of birth control, while continuing to condemn it on both moral and physiological grounds. Much of that condemnation was remarkably imaginative in character.

While giving us a detailed and scholarly account of what many individuals wrote and said, Soloway has provided a somewhat less telling picture of the demographic changes that occasioned much of the debate; no graphs or tables are offered, all statistics being presented in narrative form. Line graphs contrasting the birth rate and family size of different social classes would have been particularly illuminating. In general, however, this is a thoughtful and informative, if not particularly succinct, history of the birth control and population question in England.

ANDREW ROSEN
St. Mary's College of Maryland

F. M. L. THOMPSON, editor. *The Rise of Suburbia*. (Themes in Urban History.) Leicester, England: Leicester University Press or St. Martin's Press, New York. 1982. Pp. xii, 274. \$40.00.

DEREK FRASER, editor. *Municipal Reform and the Industrial City*. (Themes in Urban History.) Leicester, England: Leicester University Press or St. Martin's Press, New York. 1982. Pp. x, 165. \$32.50.

By the publication of his study, *Victorian Suburb: A Study of the Growth of Camberwell*, in 1961 and the

vigorous promotion of research on all aspects of the history of towns and cities, Jim Dyos pioneered the systematic study of urban history in Britain. It heralded an explosion of theses on urban processes, variously defined, in numerous locations equally unmemorable as the unattractive suburb that was the subject of his own tour de force. Shortly before his death, Dyos conceived of a series of publications that would focus upon a theme within the boundaries of urban history, would present the results of recent research on selected theses, and would contain an introduction that expanded upon the chosen theme in the light of the detailed studies. Of the volumes under review, *The Rise of Suburbia* traverses Dyos country *par excellence*. It contains competently researched studies of metropolitan, suburban, and greater London by J. M. Rawcliffe, Michael John, and M. C. Carr and allows comparisons of intrasuburban development within London. London's development is contrasted with the building and estate development of the middle-class suburb of a northern industrial city—Headingley in Leeds—the history of which is carefully delineated by Colin Treen. Three of the contributors deal mainly with the second half of the nineteenth century. The essay that describes the development of Bexley, Kent—the metropolitan suburb—alone extends into the twentieth century and concentrates mainly upon the interwar period. The risk that these microscopic disquisitions on such forgettable subjects as suburban development in outer West London, for example, would be ignored or forgotten is removed at a stroke by F. M. L. Thompson. He rescues them from a respectable but unrewarding obscurity by encapsulating the results of the research in a lucid and perceptive introduction that highlights the significance of each contribution, both by comparison and by providing a wider methodological, historiographical, chronological, and spatial framework.

An element common to all four pieces is an initial examination of population growth in relation to suburban development. Fundamental questions are raised regarding the proportion of net population increase occurring in each suburban location and concerning which *parts* of the surrounding space experienced the transformation into suburban residential areas: when, by what stages, and by whom; the explanation for it; and the determinants of the particular shape of the resulting suburb. Inevitably, the historical microscope concentrates, though not entirely, upon the uniqueness of the origins and development of each suburban specimen. But generalizations do emerge from the research, and Thompson's introduction underlines them: the importance of rural pre-urban development as a major influence on all aspects of the subsequent classic suburbanization process coupled with the limitations placed on the decisions of landowners and

developers by market forces; the diverse ways in which transport services contributed to suburban developments in various situations; the social heterogeneity characteristic of suburban populations combined with self-zoning tendencies; and the protracted discontinuity of growth in any particular neighborhood. Thompson's distillation of his contributors' research considerably enhances the value of the individual chapters, but he achieves more than this. For by exposing and questioning the implicit assumptions on which the research he summarizes rests, specifically, that the creation of further suburbs after 1830 became virtually a self-sustaining activity until political regulation arrested the process, Thompson directs attention to a critical stage in the very early history of suburbs, about which we know little. By offering hypotheses of his own to explain the origins of suburbanization, he transforms a review of recent research into an agenda for the future.

It would be surprising if a similarly thought-provoking commentary accompanied the collection of essays included in *Municipal Reform and the Industrial City*, edited by Derek Fraser. This is so not because the focus is provincial rather than metropolitan but because neither Leeds, the subject of Barber's essay on urban administration between 1830 and 1900, nor Bradford, whose municipal government between 1840 and 1860 forms the subject of Elliott's contribution, has stimulated as much interest among historians as Manchester, the "shock city" that captured the imagination of contemporaries and is the location for the third piece in this collection. Whereas the essays by Elliott and Barber are thoroughly researched, principally exemplifying or particularizing aspects of the urban process, Gatrell succeeds in engaging interest at different levels. He analyzes the exercise of political power by the Manchester merchant elite between 1790 and 1838. He explores the sources of their support and emphasizes the fragility of the Liberal hegemony in Manchester until incorporation, for which the group had pressed, ultimately precipitated the eclipse of a group whose economic philosophy has long inhabited center stage in British economic and social history of the early nineteenth century. The study of municipal reform also possesses a long history, but the enormous historiography that it has generated limits the scope for innovative research and interpretation. The result is that Fraser, lacking a basis for producing an editorial *éclat* to compare with that presented by Thompson, falls back on a view he has expressed elsewhere, that the so-called municipal revolution heralded by the 1835 Municipal Corporations Act is better understood as a transformation of the principles embodied in the Act.

Remote from the familiarities of the history of

Victorian municipal reform, suburbanization offers most promise, on this evidence, of lively debate and research of increasing significance to the historiography of urban history.

ROY CHURCH
University of East Anglia

STUART A. COHEN. *English Zionists and British Jews: The Communal Politics of Anglo-Jewry, 1895–1920*. Princeton: Princeton University Press. 1982. Pp. xv, 349. \$32.50.

This is a pioneering study of the struggle between Zionists and anti-Zionists for the leadership of Anglo-Jewry at the turn of the century, and it should serve as a corrective to the exaggerated self-image of the Zionists concerning their communal achievement in those years. Stuart A. Cohen's meticulous analysis of the varied source material demolishes this myth; he also demonstrates rare objectivity among historians of Zionism by unhesitatingly presenting a rounded picture of Zionist failures. He convincingly argues that "most leaders of the English Zionist Federation (EZF) evidently sought communal prestige and influence, whether at a national or a local level, for its own sake" (p. 126); in comparison with the anti-Zionists, they were "unimaginative" and "uninformative" (p. 158), suffering from "doctrinal poverty" (p. 159). He remarks with particular pungency: "From an ideological perspective, then, Anglo-Jewry Zionists were a barren assemblage who paid no more than intellectual lip service to the ideal in which they professed to believe" (p. 159). Nor did they make any original contribution to Zionist thought.

The anti-Zionists too are accorded distinctly fair treatment. Cohen rejects the Zionist cliché that their opponents were motivated by personal fear covered by public respectability. Rather, they were in reality motivated by intellectual conviction, personal interest, and social manners (p. 102). The Zionists consciously distorted their opponents' image by claiming that the latter wished only to fudge their Jewish identity. If anything, Cohen presents the anti-Zionist leadership (Israel Abrahams, Claude Montefiore, and Lucien Wolf) as far more attractive than that of the Zionists (Joseph Cowen, Rabbi Moses Gaster, and Leopold Greenberg). The anti-Zionists were more intellectual, certainly more skeptical. Abrahams claimed (1897) that "the Jewish state is more likely to harm Judaism than to destroy anti-Semitism" (p. 167). Indeed, the Zionists were the real assimilationists, since they wanted to constitute a nation like all others. But the anti-Zionists were bound to be defeated when their main ideological weapon, the charge of "dual loyalty," was blunted

and it became clear in 1917 that there was no contradiction between Zionist ambitions and British imperial interests. Nevertheless, their defeat was somewhat of a surprise. It came in June 1917 when the Board of Deputies of British Jews voted to censor the anti-Zionist manifesto published by the Conjoint Foreign Committee the previous month. This is convincingly described as a change in the delicate balance between the government, the Zionists, and the anti-Zionists, but the author emphasizes that it did not mark a turning point in terms of communal politics. Weizmann was wrong to hope (like Herzl earlier) that resistance to Zionism would disappear after the British government's expression of clear-cut support for Zionism. Curiously, but significantly, in late November 1917 the majority at the Board of Deputies rejected the proposal to assist the EZF to establish a National Home in Palestine. The Zionists had to go back to the policy of infiltration of Anglo-Jewry's institutions in order to conquer the community (which they finally achieved only during the Second World War). In view of Cohen's impeccable scholarship, it becomes clearer that Weizmann used cleverly the identification between Zionism and imperial interests, playing down the strategy and notion of the "conquest of the community," but he never achieved a communal victory.

One criticism that one would be tempted to make relates to Israel Zangwill, whose interesting comeback to Zionism is left out, probably because of its lack of relevance to the communal struggle. Apart from Weizmann, Zangwill is the only interesting and provocative personality among the Zionist leadership (his Ito period was indeed a Zionist aberration, not an anti-Zionist manifestation), as is revealed in the pre-1914 part of the book. In particular, one should single out his extreme line on the Palestine Arabs, whom he advocated transferring to other Arab countries, an idea that later influenced Vladimir Jabotinsky. More information about other figures, such as the divided Rothschilds, awaits further opening of the archives. Cohen's study is an example of clear presentation of historical scholarship.

JOSEPH HELLER
Hebrew University of Jerusalem

F. S. NORTHEGE and AUDREY WELLS. *British and Soviet Communism: The Impact of a Revolution*. London: Macmillan; distributed by Humanities Press, Atlantic Highlands, N.J. 1982. Pp. viii, 280. Cloth \$31.50, paper \$14.75.

After giving a brief sketch of British relations with tsarist Russia, F. S. Northedge and Audrey Wells

address the main themes of British policy since 1917. During the 1920s, the authors argue, the Soviet example helped show the British people and their governments how not to attack the social issues of their time. Instead of being grateful, the British embarked on an irrelevant feud with the Soviets, setting back disastrously the normalization of relations that would have served British interests. Later, mismanagement of relations by Britain and France, after years of indecisive dealing with Nazi Germany, presented Stalin with alternatives that led him to conclude that Russia's advantage lay in signing the pact of 1939 with Hitler. When the Anglo-American-Soviet alliance was finally forged by wartime necessity, there were few real roots of mutual sympathy or shared ideals with the Russians. After the war, Russia, like France in 1919, sought doggedly to ensure the security of vulnerable borders. The British and American reaction was understandable, to be sure, but Northedge and Wells insist that in many instances the Russians have had a better case than superficial judgments imply, often better than that of the Western states they have had to deal with.

In Britain, fear and mistrust of Russia, which had so dominated opinion in the nineteenth century, were never fully exorcised, but in many quarters, particularly during the Second World War, Russophobia virtually ceased to exist. Even earlier, of course, especially during the thirties, left-wing intellectuals had welcomed the Soviet Union. They saw its system as a possible antidote to the drift inherent in the capitalist scheme of things, and they viewed the Russians as the only effective opposition to Nazism. Disenchantment for them set in with the purges of the mid-thirties and, more sharply, with the Nazi-Soviet Pact of 1939. Rejection faded during the wartime struggle, but postwar intellectuals became as cool to the Soviet Fatherland as did the generality of the British people. Even the British Communist party, none of whose ideological twists and turns made it a significant force on the British left, by the late seventies was maneuvering to hold the USSR at arm's length.

Finally, Northedge and Wells observe that from their beginnings in the sixteenth century British commercial contacts with Russia have never been of major scope. Since 1917 they have been few, they have been particularly sensitive to the state of political relations, they have made little contribution to the improvement of the Anglo-Russian political climate, and they have not acted as significant sources of Anglo-Russian differences.

The conclusions drawn from this examination are not surprising. The authors stress that little can come from attempts to belittle the Soviet Union or to deny it recognition as a superpower. Although

they insist that the deterrent structure of the NATO pact is a necessity for Britain, they warn that the legitimacy of Russia's international position needs to be understood. The Soviet Union, they point out, has as strong an interest in peace as Britain or any other Western power. This calm and thoughtful little work is a welcome antidote to the overheated rhetoric currently in such frequent use. It would be even more welcome if one could imagine the early appearance of a Soviet counterpart.

HENRY R. WINKLER
University of Cincinnati

GEORGE R. HEWITT. *Scotland under Morton, 1572-80*. Edinburgh: John Donald; distributed by Humanities Press, Atlantic Highlands, N.J. 1982. Pp. vii, 232. \$29.50.

After so many books on John Knox, Mary, Queen of Scots, and her son, James VI, in recent years, it is refreshing to read a book on James Douglas, Earl of Morton, who was one of the regents during the minority of James VI. Although George R. Hewitt has attempted neither a biography of Morton nor a complete history of Scotland between 1572 and 1580, he divides his book into five narrative chapters and five thematic chapters. The former cover the entire lifespan of the Earl of Morton and give the important highlights, especially his dramatic execution. The latter explain the regent's leadership in the Scottish kirk, administration of the borders, as well as domestic and foreign policy.

This overall organization of the book creates a number of problems. The first four narrative chapters assume a tremendous knowledge of the pre- and post-Reformation era in Scotland. Many of the ranking nobility, churchmen, and large landholders are paraded through with their likes and dislikes of Morton. Morton's success as regent seems to have depended more on his cunning and supposed relationship with Elizabeth than on his support among his own kinship groups and recipients of benefactions. Some of the lists and benefactions would be better placed in appendixes than in the narrative. The first two chapters flow smoothly without subdivision; then in chapter 3, on page 44, there appears "The End of the Regency"; and subdivisions appear in chapter 4 as well.

This same sort of subdivision fails to appear in chapters 5 and 6 on the Scottish kirk. It reappears in chapters 7 and 8, disappears in chapter 9, and surfaces again in the final chapter. Obviously, this creates questions about the organization of the book. Is this simply the problem of a PhD thesis going to press? In any case, Hewitt has admirably analyzed the Scottish kirk under Morton without

making too strong a case for Morton's support of the episcopacy. A good case is also made for Andrew Melville's dislike of the regent's Anglophile policies with regard to the kirk. The chapters on domestic and foreign policy are good as far as they go, but they leave gaping holes and constitute only superficial coverage.

In the final chapter, Hewitt returns to the narrative and explains why the Earl of Morton was executed by a power-lusting Earl of Lennox. Here the book is at its best, although the reader is left with only a very brief conclusion that fails to do Morton or Scotland justice.

The book will be an important addition to sixteenth-century studies in Scottish history for scholar and graduate student alike. However, undergraduates and popular-history readers will find it too heavy and hard to follow. The bibliography will be instructive to all.

CHARLES H. HAWS
Old Dominion University

W. A. SPECK. *The Butcher: The Duke of Cumberland and the Suppression of the '45*. Oxford: Basil Blackwell; distributed by Biblio Distribution Center, Totowa, N.J. 1981. Pp. x, 230. \$25.00.

W. A. Speck has provided us with a judicious account of the course and suppression of the 1745 rising together with a declaration of faith concerning the episode's significance.

The narrative of the rising and its aftermath is soundly based on a range of primary material and involves a careful appraisal of previous work. For a narrative both readable and scholarly, one could do worse than this and, considering the mythology that the '45 has generated over the past two and a half centuries, could do a great deal worse. The rising put to a final test the appeal of the Stuarts in the United Kingdom, a test it failed. The *coup de grâce* was administered by the duke of Cumberland—"the Butcher," a nickname current in both England and Scotland shortly after Culloden.

Did Cumberland deserve the title? The answer seems to be both yes and no. In the ruthless treatment meted out to highlanders immediately after the battle and during the subsequent "pacification," Cumberland was both obeying his own military instincts and responding to pressure from the civil government and the force of popular opinion. Englishmen and lowland Scots alike regarded highlanders as barbarians and "vermin" who ought ideally to have been exterminated but who, in an imperfect world, had at least to be taught a very severe lesson. The duke was doing the nation's dirty work and was the obvious one to carry the blame when the nastier details became known.

But narrative apart, Speck's avowed aim is to combat what he sees as a tendency to portray the Hanoverian regime as "a vulnerable and exposed gang of adventurers and free-booters whose collapse was ardently desired by most subjects of the first two Georges" (p. 3). He accepts the anti-Jacobite spasm, which after Prestonpans ran through all levels of society, as conclusive in discrediting this proposition. And, of course, by itself it does nothing of the kind. Under Walpole there certainly had been discontent and even disaffection. After his fall, there had appeared no visible resurgence of enthusiasm for the regime. But to let fancy dwell on a vague Stuart alternative as remote as the Second Coming was one thing. To risk what was portrayed as "Slavery and Bigotry, Idolatry [*sic*] and Superstition, under a Popish Arbitrary Power sent from Rome," imposed moreover by highland broadswords with French encouragement, was altogether another prospect and quite enough to panic the country into supporting the establishment. Speck cites 1688 as an example of a ruling order's capacity to cause a king's downfall merely by withdrawing its support. The survival of the regime in 1745–46, he argues, indicates support for it, either active or passive, and therefore its general acceptance. This might well be correct but is not self-evidently so. The ruling order in 1688 saw itself as declining to support popery despite royal encouragement. In 1745 they were resisting what looked very much like an attempt to impose popery from outside. It did make a difference.

Most peoples have been, and still are, ruled by relatively small elites doing quite nicely for themselves. Hanoverian England was no exception. The fact is not particularly significant, and most accepted it with or without complaint. Nor is it remarkable that in the last resort, whatever promises were made by Charles Edward Stuart and his father, both England and Scotland preferred the status quo.

P. W. J. RILEY
University of Manchester

R. B. MCDOWELL and D. A. WEBB. *Trinity College, Dublin, 1592–1952: An Academic History*. Foreword by F. S. L. LYONS. New York: Cambridge University Press. 1982. Pp. xxiii, 580. \$74.50.

Trinity College, Dublin (TCD), is approaching its four-hundredth anniversary in 1992, and there are many Irish, British, and Irish-Americans who would never have believed it, at least in its present, still recognizable form. For TCD was and, to some extent in long-memoried Ireland, still is an ambivalent institution, at once the pride of Ireland in the wider intellectual world and the hated symbol of the Protestant Ascendancy. R. B. McDowell and D. A.

Webb, eminent current professors of the beloved alma mater, have tried to bridge this paradox by writing an *academic* history, which means that whenever they mention politics they apologize and promise to be brief. In Ireland, however, everything is political—religion, education, even sport and music—and politics, like cheerfulness with Dr. Johnson's would-be philosophic friend, keeps breaking in. Here this is all to the good, because the political tension raises what might have been a pious chronicle of academic worthies and domestic bickering to a work of much wider interest, the story of an international university struggling to maintain cosmopolitan standards of scholarship in a *piéd noir* colonial environment.

The book is of still greater interest to historians of higher education and, although not quite so novel in approach as Provost Lyons in his judicious foreword suggests, does raise and answer modern questions about student numbers and social origins, what subjects they were actually taught and by what methods, where and how they lived, and how they amused themselves. Trinity College with its affluent fellows never quite sank to the depths of endowed idleness of eighteenth-century Oxbridge. On the contrary, it maintained a good general education, including natural science and philosophy as well as classics and divinity, for its often teenaged students. It pioneered the public examination system, both for its undergraduates and for its relatively fair and open selection of scholars and fellows, and, long before London University's external degrees in 1858, developed a provision for mature students with little or no attendance that amounted to a much-needed system of adult higher education.

When Royal Commissions were appointed for Oxford, Cambridge, and TCD in the 1850s, the Dublin institution came out of it with much less criticism than the English universities. Trinity College for its size could boast as respectable a list of famous sons as any university down to the breakup of the Union: men as eminent as Bishop Berkeley in philosophy, Sir William Hamilton in natural science, Ernest Dowson in literary criticism, and John Joly in every kind of applied science from color photography through plant physiology (the ascent of sap) to the geological thermal cycles underlying the theory of continental drift.

This monumental academic history nonetheless illustrates what a tragedy for Ireland and higher education was the Act of Union of 1800 and how nearly tragic for TCD, paradoxically but for Eamon de Valera's later help, was its collapse in 1922. Sadly, as with all Irish success stories, tragedy keeps breaking in.

HAROLD PERKIN
National Humanities Center
Research Triangle Park, North Carolina

JOSEPH V. O'BRIEN. *"Dear, Dirty Dublin": A City in Distress, 1899–1916*. Berkeley and Los Angeles: University of California Press. 1982. Pp. xiv, 338. \$32.50.

Among the many virtues of Joseph V. O'Brien's scholarly, well-illustrated book is its breadth of coverage. It examines private philanthropy (hampered by bitter sectarianism), police, crime and prostitution (vital reading for all followers of Bloom's progress through Nighttown), recruitment for the British army, reactions to the Boer War, the arts (the Abbey Theatre notwithstanding, Dublin was, he argues, culturally retarded), sport (despite the efforts of the Gaelic Athletic Association, soccer was far more popular than Gaelic football or hurling), municipal elections, the composition of the corporation, and the 1916 uprising. But O'Brien's main theme is "the nether world of tenement and slum, of the poor and unemployed" (p. xi). It was the ineluctable nature of Dublin's poverty that doomed it to vie with Liverpool as the dirtiest and most diseased of Britain's major cities: poverty that, despite the negligible growth of its population, forced one-third of the inhabitants to live in overcrowded, one-room tenements; and poverty that lent such urgency and poignancy to the labor unrest of 1913, during which twenty thousand Dubliners were involved in strikes or lockouts. It is an unrelieved story of urban pathologies, and O'Brien recounts it lucidly, compassionately, and without rancor.

In the poverty of its unskilled labor Dublin was but an extreme example of the urban misery that Seeborn Rowntree and Charles Booth discovered in England. Perhaps of greater interest to urban and social historians are the differences between Dublin and other major British cities. Unlike most British (and American) towns, Dublin in the nineteenth century failed to incorporate the suburban townships to which the middle classes fled. Together with a stagnant economy this resulted in a low tax base that made it impossible for the corporation to qualify for low-interest, long-term government loans. By 1916 the corporation had a total indebtedness of £2.75 million. This partly explains why Dublin, unlike the English towns investigated by Peter Hennock and Derek Fraser, failed to generate a great civic awakening and remained "cribbed, cabined and confined" (p. 15).

But it was not poverty alone that discouraged municipal progress and civic consciousness. In 1906 the corporation, many years after major English cities, finally inaugurated its main drainage system. Alderman Kelly, a member of the secret Irish Republican Brotherhood, seized the occasion to cast the Union Jack into the River Liffey, a highly symbolic act. Indeed, Dublin's status as a colonial capital in a period of militant nationalism made it

inevitable that national politics would occupy the corporation's energies and divert its attention from its manifold urban ills.

O'Brien's detailed statistics occasionally intrude on his otherwise readable and compelling analysis, and one would like to know more about the beleaguered medical officer, Sir Charles Cameron, and the social philosophy of the churches. But these are minor criticisms of a provocative and compelling work that authoritatively fills a shameful gap in urban history. Certainly no one reading his "warts an' all" account will ever again harbor uncritical nostalgia for the Periclean Dublin of Joyce and Yeats.

ANTHONY S. WOHL
Vassar College

JOHN W. SHAFFER. *Family and Farm: Agrarian Change and Household Organization in the Loire Valley, 1500–1900*. (SUNY Series on European Social History.) Albany: State University of New York Press. 1982. Pp. 258. Cloth \$39.00, paper \$12.95.

Because families provide education, insurance, and pensions to their members at low transactions costs, farm families normally have privileged access to their own supplies of labor and capital compared with other production organizations. Where the efficient size of a farm has exceeded the capacity of nuclear families to supply these inputs, farm families have sometimes enlarged themselves by associating adult offspring and collateral relatives as a means of securing economies of scale without incurring the high transactions costs of dealing with strangers. John W. Shaffer's book analyzes the history of one such extended family association, the *communautés* of the peasants of Nivernais. *Family and Farm* is a carefully thought-out account of how economic and legal change altered the relative advantages of the extended joint family farm in rural Nivernais between 1500 and 1900. As such, it is a contribution to both family history and the economic history of peasant farming in central France.

The farming *communautés* of the Nivernais were family partnerships that pooled land, capital, cash, and labor to farm and maintain the integrity of large holdings. The institution may have originated in rules that prevented serfs from dividing land held in *mainmorte* and required the heirs to reside on the holding. A less stringent tenure known as *bordelage* contained similar stipulations, which tended to encourage the holding of land by joint families. Shaffer shows that these legal conditions were less important than economic ones in maintaining the extended-family partnerships. By the end of the seventeenth century, legal restraints on residence and division had been largely voided. A more

powerful reason for the survival of joint families was the parents' desire to insure their retirement by imposing *communauté* on the heirs as a means of maintaining the integrity of the holding as a farming unit. When the revolution eliminated parental power to donate land to the heirs of their choice and on conditions of their own choosing, the institution swiftly succumbed to the claims of heirs to their *légitime*.

Although land ownership by joint families disappeared before 1850, the joint-family farm in *métayage* hung on to the end of the century. Large sharecropper farms emerged in the seventeenth century when the burden of taxes, heavier in this part of France than in the frontier districts, forced peasants to sell their holdings to noble and bourgeois purchasers, who let the land back out in holdings large enough to require the labor of several adult males. Since labor costs were borne exclusively by the *métayer* and because many families had previously owned land in *communauté*, the transition to extended-family sharecropping was easy. In districts where labor was more readily available, as was evidently the case in nucleated villages, joint-family sharecropping failed to develop, which suggests that the availability of labor was a determining factor in the regional incidence of the institution. It is clear that the families were formed in response to the scale economies of farming. Any change in technology or in crop mixes that altered the size and pattern of labor demand was bound to affect the size and form of the farm families.

The final section of the book reviews the economic changes in rural Nivernais and their effects on sharecropping families: improvements in transportation, advances in husbandry, and changes in crop mixes altered the pattern of demand for farm labor and undermined the advantages held by the joint family in low-yield subsistence farming. Extended families disappeared because their privileged access to labor ceased to provide competitive benefits. Stem and nuclear families supplanted the joint family, just as leases supplanted sharecropping. The account of agricultural change in this section is cogent and sure. Shaffer makes intelligent use of the statistical evidence bearing on agrarian change in this period by providing a painstaking reconstruction of land use by individual farms in two communes. This is technically the best part of the book. It points the way to a proper statistical investigation of French farming in the middle years of the nineteenth century. Another valuable contribution that points to further work is Shaffer's discussion of the mistrust that poisoned relations between *métayers* and their landlords, which effectively limited their ability to cooperate in raising yields. These are central and as yet little-studied features of rural Europe in the nineteenth century.

In sum, this is an important book. Students of rural France in the nineteenth century could well begin their work by studying it, both for its conception of the way things changed and for its craftsmanship.

GEORGE W. GRANTHAM
McGill University

ROBERT DARTON. *The Literary Underground of the Old Regime*. Cambridge: Harvard University Press. 1982. Pp. ix, 258. \$16.50.

Robert Darnton has invested much of his scholarly energy writing important essays on a universe of late eighteenth-century hacks, journalists, printers, booksellers, and readers whose production, dissemination, and consumption of clandestine literature in prerevolutionary France may well have contributed to the creation and success of radical Jacobinism. Darnton's essays dealing with the sociology of Grub Street, under-the-cloak pamphleteers and book peddlers, police spies turned Girondin leaders, and wandering pressmen, have illuminated a civilization on the eve of cataclysmic political and social change. Darnton, of course, also has analyzed the book as an instrument of the transformations occurring in France during the 1770s and 1780s; and his articles on the writing, manufacture, distribution, and sale of books—particularly illicit books—are intended to respond to the classic questions: "How deeply did the Enlightenment penetrate into French society? How much did radical ideas contribute to the destruction of the Old Regime? And, what were the connections between Enlightenment and Revolution in France?" Yet in his analyses Darnton is too wide-visioned a historian to neglect political events, economic crisis, and social dysfunction. As he admits in the preface to the present volume, a collection of five previously printed essays (two abridged) and one lecture, he is contributing to the creation of a mixed genre of scholarship, the "social history of ideas." Rejecting the classic techniques of intellectual history, he has plunged into the business records and correspondence of an important publishing house, he has skillfully used the archives of the Paris printers' and booksellers' guild, he has sifted through hundreds of police dossiers, and he has read *Les fastes de Louis XV* as imaginatively as an article from the *Encyclopédie*.

Since five of the pieces in *The Literary Underground of the Old Regime* are accessible in university libraries and the sixth is expanded on in Darnton's monograph, *The Business of Enlightenment: A Publishing History of the Encyclopédie, 1775–1800*, Harvard University Press is aiming the present collection at an audience of nonspecialists and general readers. This is an excellent idea. There is unity to the collection;

Darnton is a superb storyteller who writes engrossingly and is unafraid to take chances. The essays were written between 1968 and 1977. They include Darnton's major piece, "Reading, Writing, and Publishing in Eighteenth-Century France," in which he criticized the techniques of quantifiers and historians of private libraries and "The High Enlightenment and the Low-Life of Literature in Prerevolutionary France," which is Darnton's socio-intellectual comparison and contrast of the giants of the *philosophe* generation that died around 1780, their vapid heirs apparent, and an embittered literary proletariat that found no recognition in the dying Old Regime. Three career studies follow: that of Brissot, turned police spy after his arrest in 1784; Le Senne, literary chameleon—*pauvre diable*—*neveu de Rameau* pamphleteer trying to land commissions from the Société Typographique de Neuchâtel; and Mauvelain, seller of clandestine books from Troyes. Rounding out the essays is an account of work processes at the Neuchâtel shop.

As Norman Hampson observed in *The New York Review of Books* (October 7, 1982, pp. 43–44), Darnton may fit writers like Voltaire, Marat, and Mercier too neatly into defined social categories, and at times he substitutes what writers "must have felt" for what they actually wrote. Reading books as a force for change, Darnton may have to exercise more patience than he seems willing with works that were not *chroniques scandaleuses*—perhaps even religious controversy. And I am more sanguine than he is about the possibilities for synthesizing quantitative and demographic studies based on the private libraries of professionals. This said, Darnton has clearly unearthed a human, multifaceted Enlightenment. What lies ahead is a systematic, compelling book on the cultural origins of the Revolution. Darnton surely possesses the vision to write it.

RAYMOND BIRN
University of Oregon

RAYMOND HUARD. *La préhistoire des partis: Le mouvement républicain en Bas-Languedoc, 1848–1881*. Foreword by LOUIS GIRARD. Paris: Fondation Nationale des Sciences Politiques. 1982. Pp. 520. 200 fr.

Raymond Huard has written a judiciously conceived, thoughtfully stated book, a work warmed by his open affection for an intransigently rebellious *pays*. "Le Gard est un département 'difficile,'" he recalls proudly (p. 17). Like Louis Girard, who contributed an excellent preface, and Maurice Agulhon, whose help is acknowledged, Huard chronicles, indeed celebrates, the emergence of a republican majority.

Huard unites an *érudit local's* respect for the

particular with considerable theoretical refinement: "Events shape strategy, propaganda, and sometimes even ideology. Inversely, it is according to these that it (a party) assimilates, integrates, and responds to political contingencies" (p. 26). He writes the "pre-history" of a party, having read, one senses, all one must read about praxis. He is cognizant of what Gramsci thought parties were but not unduly burdened by this knowledge. His own scholarly independence suggests the self-assured selectivity of the Gard's republicans.

Huard describes the challenges the department's unique religious character posed to those principled, but alert and supple, militants. The Gard was fully one-third Protestant, and this substantial minority was geographically concentrated and socially differentiated. There were, in the Cévennes, even Protestant peasants; but the rural and urban poor were predominantly Catholic. Early in the nineteenth century one could assume a Catholic to be a legitimist and a Protestant, if not a republican, at least a man of the left. Until 1848, a restricted franchise protected the hegemony of privileged liberals. Thereafter, universal suffrage naturally tempted conservatives to insist that confessional and political commitments were inseparable.

Republicans sought to dissociate those choices—their anticlericalism constrained by the dangers they saw in making a left-of-center vote seem an act of apostasy. Republicans were increasingly successful in this strategy, Huard finds, as economic development freed rural populations from reliance on Catholic landlords. He uses election returns to good effect, showing, paradoxically but plausibly, that, although religion did persist in correlating with voting patterns in mixed communes, the more homogeneously Catholic areas were not the most reliably monarchist. He speculates, too, that the precocious socialism of Nîmes, in the Third Republic, might be explained by the preference of Catholic weavers for their enemies' enemies: better a socialist than a bourgeois radical.

There were, of course, tensions within each movement. Catholics were able to disengage from legitimism when they found in Napoléon III a more effective patron of Pius IX. Legitimists were themselves divided between men concerned above all with the defense of property, who made themselves available for coalition with other men of order and eventually became reconciled to a Republic that promised social peace, and their more stirring Tory radical associates, whom Huard calls the *Montagne blanche*.

Protestants were also split between men of order and of movement; as well as between conciliatory deists, like the appealing Germain Encontre, a republican victim of the coup of 1851, and those less tolerant, like the Protestant mayor who harangued

his village in 1872: "Hors du christianisme, point de salut; hors de la République, point de bonheur" (p. 366). Amid all his carefully marshaled data, Huard lets shine some marvelous characters.

But it was the republicans alone who made of their divisions a broadly appealing, powerfully inclusive synthesis. Huard distinguishes between a legalist, gradualist wing of the party, committed to popular sovereignty, and an "expressionist" group that was unwilling to rely on moral suasion, prone to insurrectionism, and frankly jacobin. Each developed forms of association appropriate to its conception of politics: circles or clubs for the former and secret societies for the latter.

Huard, rightly, identifies the Second Empire as the time of critical coalescence for the republicans. The circles become politicized, the societies proselytized openly. The unevenness of repression in the early fifties strengthened the popular and *petit bourgeois* character of the movement; and a moralizing, hortatory socialism, proposing not to suppress the rich but to curb their illegitimate political influence, appealed to many workers and also to the *nouvelles couches* created by imperial prosperity. Huard's observations are fine: he notes that small manufacturers and shopkeepers, though often politically active, seldom held office. The *nouvelles couches* were represented by their sons, men of letters or professionals—an instance of the intergenerational mobility that blunted class antagonism within republican ranks.

Huard's story is of a center holding and growing, of a movement appropriating the center and presenting itself as uniquely able to resolve social, economic, and religious conflicts. A republic, its supporters held, could both express and discipline, legitimate and moderate, popular grievances. It was the best means to that end, and the republicans held fast to it. But the movement held the republic to be, also, an end in itself; and Huard sees that commitment as essential to their triumph.

KATHERINE AUSPITZ
Harvard University

GEORGE D. SUSSMAN. *Selling Mothers' Milk: The Wet-Nursing Business in France, 1715–1914*. Urbana: University of Illinois Press. 1982. Pp. xi, 210. \$16.95.

This well-crafted monograph does two things. It writes the institutional history of wet-nursing in France, beginning with the Provost of Paris's 1685 decree on the "disorders and abuses committed daily" in recruiting newborn infants for transport to far-distant, paid, rural nurses. The story concludes with the Roussel Law of 1874 and the precipitous decline of paid wet-nursing around the time of the

First World War. Sussman meticulously tells this administrative tale, evaluating material from the archives of the Paris Public Assistance, from the papers of the "Bureau des nourrices," from other series in the National Archives, and from extensive eighteenth-century material in the National Library. It is unlikely that anyone will need to go over these documents again for the purpose of writing the story of mercenary nursing "from the top down."

Secondly, however, Sussman wants to write about wet-nursing as seen "from the bottom up," which is to say, from the viewpoint of the parents who sent their newborn away to the countryside in this remarkable French practice; from the viewpoint of the nurses, as he explores their motivations for taking on these little babies; and perhaps from the "viewpoint" of the nurslings themselves. Sussman has much fine material on how the babies were treated while away, on their weaning and toilet training, and on the terrible mortality that prevailed among them, far higher, it must be said, among the illegitimate than among the legitimate. This part of the book evaluates the extensive medical literature on wet-nursing and draws on provincial and local archives for a number of case studies from the eighteenth and nineteenth centuries.

Students of the history of the family will relish, in particular, this "from-the-bottom-up" approach; for it addresses a question that has intrigued family scholars ever since the 1950s: why did French parents treat their newborn with such apparent coolness, sending them away for a year or two to an unknown household (*meneurs* being usually involved as intermediaries) from which the infant had at least a one-in-three chance of never returning? What of motherly love? What of maternal-infant bonding? What of that whole complex of close sentiments spun about the child that we associate with the nuclear family?

Sussman treats these questions a bit too gingerly for my taste and appears, for example, to find nothing remarkable in those parents who would send their children away and then not pay the wet-nurse, thus risking some potentially fatal discontinuity in the care of their child. Parental "poverty" for him is the explanation, but the social classes that sent children to nurses—the small artisans and shopkeepers of the cities—were by no means the poorest. If the fathers and mothers suddenly stopped paying, it was not necessarily because they had no money but because they chose to spend it on other things.

He does, however, consider each of the three major explanations for the sudden decline around 1914 of sending children to rural nurses: (1) the "cultural" explanation, which I myself, among others, have advanced, namely that a change in family attitudes toward small children henceforth made

their removal from the household inconceivable; (2) the "technical" explanation, which argues that the invention of safe handfeeding practices made it possible to keep at home babies whom mothers, for whatever reason, did not wish to breastfeed; (3) the "socioeconomic" explanation, which argues that in the eighteenth and nineteenth centuries work outside of the house took on a singular importance for married women in France and therefore that these women were obliged to send their babies away. When in the early twentieth century much of this extradomestic work for women fell away, so did the need for paid mercenaries. Sussman inclines toward this latter explanation, while admitting that among middle-class women in cities the use of mercenaries had come to an end long before. Which of the explanations is correct remains unclear. But this modest, well-researched study clears away much of the underbrush for other scholars who will want to know about the emotional significance of this singular custom.

EDWARD SHORTER
University of Toronto

PATRICIA O'BRIEN. *The Promise of Punishment: Prisons in Nineteenth-Century France*. Princeton: Princeton University Press. 1982. Pp. xiii, 330. \$28.50.

One way to focus on the study of nineteenth-century history, particularly the history of crime and its punishment, is to emphasize the extent to which all those comfortable postulates of the Enlightenment—the malleability of human nature, the beneficent influence of social institutions, and the possibility of human progress—failed to describe the realities of the century. Nowhere was the gap between theory and practice so extensive as in the history of punishment and prisons in nineteenth-century France. Punishment under the *ancien régime* had been a form of retribution, a public ritual in which the body of the convicted person was marked, mutilated, or dismembered in expiation for his or her crime. Prison then had been merely a way station for the accused, a brief stopping place on the road to punishment or acquittal.

In contrast, French legislators and reformers after 1815 identified prison as a place of rehabilitation, a locus wherein the elements of work, discipline, silence, isolation, moral example, and exhortation would divert the punished from a life of crime. Whether in the former religious houses of Saint-Lazare or the Abbaye in Paris, or in the new structures constructed during the Restoration in Limoges, Melun, Clairvaux, and Loos, these prisons had a common function: to change the lives of prisoners by segregating them according to sex and

age, by requiring them to work, and by stamping them with a modicum of order, morality, and education.

Parts of Patricia O'Brien's subject matter have already drawn a diverse and provocative group of students. In the 1960s Louis Chevalier emphasized the apparent link between working and criminal classes at mid-century and the horror with which middle-class Frenchmen regarded the apparent increase in criminality among the lower orders. In a related fashion, historians of Marxist persuasion have seen the new mode of punishment as simply another aspect of the continuing war between the proletariat and the bourgeoisie. Michel Foucault's contribution is too complex to discuss fully here, but it treats punishment not so much as a product of economic and social relationships, but as an expression of power over the body and mind of the individual. O'Brien acknowledges all these viewpoints without adopting any particular one; her conclusion in the final chapter simply summarizes her more detailed findings, without linking them to a particular interpretation.

One of O'Brien's primary goals is to focus on the prison population, rather than on their guardians or the legislators, administrators, and reformers who wrote about them. Although her footnotes are full of *mémoires, conclusions, études, traités, rapports*, and *enquêtes* by these latter groups, she largely succeeds in keeping the focus on the prisoners themselves. Thus her treatment of women and youth in prisons, of the prisoners as workers, and of new prison subcultures is detailed and highly interesting. Numerous illustrations and drawings enhance the text in these chapters. Consistent with Eugen Weber's findings about the role of schools and military service, O'Brien's account depicts prisons as a homogenizing force among the prisoners, reducing or eliminating differences in dialect, custom, and attitude.

Instead of rehabilitation, the century witnessed a growing rate of recidivism that appalled contemporaries, who were unaware that it derived in part from better record-keeping and in part from prosecution for petty crimes such as begging and vagrancy, which in the eighteenth century would have earned the perpetrators merely a one-way trip out of town. The rise of recidivism led to experiments such as agricultural colonies for youths and, ultimately, the relegation of large numbers of repeat offenders to the penal colonies of Algeria, Guiana, and New Caledonia.

Top honors for hypocrisy during the century go to administrators who resolutely denied that high rates of disease and mortality indicated prisons were unhealthy places; to architects of a surveillance system that forbade released prisoners to return to

their domiciles in Paris, major cities, and areas of heavy industry or tourism; and to anticlerical republican politicians who condemned the efforts of religious groups, such as the Order of Marie-Joseph, to found houses of refuge for released women convicts. Although O'Brien does not emphasize the point, one is struck by the extent to which the nineteenth-century French prison system incorporated a gigantic contradiction. Instead of rehabilitation, it created a class of criminals, branded on the passport or *livret* rather than on the body, restricted in movement once freed, untrained for new occupations, little educated by prison teachers, and surely not inspired by the moral qualities of those who had guarded them. One finishes the volume with a bleak sense of discouragement, tempered only by O'Brien's extensive research, thorough discussion, and occasional glimpses of humane treatment or mutual support among prisoners in a system designed for frustration and failure.

JAMES F. TRAER
Adrian College

MICHEL LESCURE. *Les banques, l'état et le marché immobilier en France à l'époque contemporaine, 1820–1940*. (Recherches d'Histoire et de Sciences Sociales, number 3.) Paris: Éditions de l'École des Hautes Études en Sciences Sociales. 1982. Pp. 621.

In this important work, Michel Lescure makes an original contribution in his assessment of the role of banks in the building industry in Paris between 1820 and 1940. Based upon exhaustive archival research in the grand tradition of French academic theses, Lescure traces, in detail, transformations in the financing of the building industry and changes in the relationship between the state and its semiofficial banks. The social consequences of policies pursued by the banks and the state, the determinants of the building cycle, and the relation of this cycle to the general business cycle also receive attention. Not the least of Lescure's accomplishments has been to provide a clear framework for organizing and analyzing these complex phenomena.

Lescure distinguishes three phases. In the first phase, 1820–60, the traditional financial structure began to be transformed with the appearance of construction companies during the building boom of the 1820s; but the availability of credit for these companies, as well as for landowners and building-trades entrepreneurs, was generally inadequate. As a result of the depression of the 1840s and the revolution in 1848, the state undertook to play a greater role by creating two semipublic banks: the *Sous-Comptoir des Entrepreneurs de Bâtiment* to provide medium-term credit and the *Crédit Foncier*

to provide long-term credit. The executive officers of these institutions were state appointees, but the ownership was private.

Lescure devotes the most attention to the second phase, 1860–80, a period of rapid growth, in which banks, both the semiofficial state institutions and private banks, became the propulsive force in the building industry. Lescure shows that during this period the semiofficial institutions came to resemble the private banks, neglecting their public-service mission in favor of providing high dividends for their shareholders. The search for profits led the Crédit Foncier into operations that were not authorized by its charter and had nothing to do with its mission—Egyptian government bonds, for example. Lescure presents much new information on the large number of specialized credit institutions that appeared during the boom of the late 1870s. One of them, the Banque Hypothécaire de France briefly challenged the Crédit Foncier's virtual monopoly of long-term mortgages.

In the third phase, a period of relative stagnation, the large private financial institutions lost interest in the building industry, leaving the field to the semiofficial institutions and their satellites. Gradually the state reasserted its control over the Crédit Foncier and the Sous-Comptoir by placing good republicans in charge and requiring these institutions to conform to their charters. Ironically, the reassertion of state control relegated the banks to a passive role. Although the slowing rate of population growth in Paris lessened the need for new housing, the Crédit Foncier, according to Lescure, missed many opportunities. It gave little support to the movement to build cheap housing for the working class and even less to the building of condominiums for the middle class. Only with the arrival of the Popular Front government after 1936 was there an attempt to infuse some dynamism into the Crédit Foncier.

There is much of interest in this book for anyone seeking to understand the relationship between high finance and politics in the Third Republic. Overall, Lescure skillfully integrates a wealth of new material into a tightly organized and comprehensible whole. Although one may suspect his picture of being a little too tidy, it is nonetheless convincing.

CHARLES E. FREEDEMAN
State University of New York,
Binghamton

STEPHEN WILSON, *Ideology and Experience: Antisemitism in France at the Time of the Dreyfus Affair*. (Littman Library of Jewish Civilization.) Rutherford, N.J.: Fairleigh Dickinson University Press or Associated University Presses, Toronto. 1982. Pp. xviii, 812. \$60.00.

Despite its excessive length and persistent repetitiveness, this book deserves careful study. It is one of the more important works on antisemitism to have appeared in many years, contributing notably to an understanding of late nineteenth-century France. Based upon prodigious research—not only in the seemingly inexhaustible literature of French antisemitism but in police and prefectural archives and anthropological studies as well—Stephen Wilson's volume is unlikely to be surpassed in the foreseeable future. Its special merit is to go beyond the detection and description of antisemitic rhetoric, to attempt an understanding of what moved writers and agitators to hit out at Jews.

If Wilson's more than eight hundred pages do nothing else, they demonstrate resoundingly how pervasive was dislike of Jews in the years following the publication of Edouard Drumont's *La France Juive* in 1886. Coming from all points of the political and social compass, antisemitism manifested itself in riots and elections, dozens of periodical publications and daily newspapers, nationalist and socialist ideologies, North African politics, economic analyses of the right and left, the Catholic church, theories of race, and many other spheres as well. At the same time it was hardly omnipotent: antisemites were divided and sometimes ill-organized, and their various leagues and organizations weakened after 1900. Wilson does not satisfactorily explain this decline, although he points to republican hostility to antisemitism, particularly the activity of the Waldeck-Rousseau government in the wake of the Dreyfus crisis.

More significant is his careful analysis of the social meaning of antisemitism. At bottom, he suggests, hostility toward Jews was a way of expressing deep-seated antipathy to a dislocating and changing society. Antisemitism emerged as a coherent and widely acceptable world view when the tempo of change accelerated and consciousness of it became widespread. Antisemites shared a mythical sense that Jews had wrought the emerging order that was seen as so detestable. Addressing diverse fears, anxieties, and uncertainties, anti-Jewish ideologies explained what had gone wrong, validated older and more comfortable values, and vaguely suggested a way out of present troubles by putting down the Jews. Antisemitism created a kind of fantasy world in which Jews were central characters. Their precise role in the mythical drama varied according to the obsessions of particular antisemites. For some it was advent of rapacious capitalism, for others the onset of national decline or collectivism, and for still others the erosion of religion, the family, or traditional sexual mores. Jews were ideally suited to play these parts because they were so prominently identified with change and seemed so strikingly its benefi-

ciaries. They had been, in addition, stereotyped historically by generations of religious and other critics, to the point that even nominal friends of Jews absorbed many anti-Jewish presuppositions.

Given that Jews "were the central characters in someone else's mythical view of the world" (p. 720), there may have been few prospects for them to improve matters. Wilson nevertheless suggests that French Jews did more to oppose antisemitism than has often been thought and that they may in some wider sense have been correct to depreciate the long-term strength of the movement. Dreyfus was, after all, rehabilitated in the end, and antisemitism ultimately proved weaker than the forces opposed to it. Along the way, however, there was sometimes a fearsome price to be paid for this particular social fantasy.

MICHAEL R. MARRUS
University of Toronto

WILLIAM G. ANDREWS. *Presidential Government in Gaullist France: A Study of Executive-Legislative Relations, 1958-1974*. Albany: State University of New York Press. 1982. Pp. xiii, 304.

When François Mitterand was elected president of France, he showed little reluctance to make full use of the powers of the presidency of the Fifth Republic, powers whose magnitude he had bitterly criticized many years before. But was the Fifth Republic originally conceived as presidentialist? And if those who framed its constitution thought they were establishing a parliamentary system, why did it evolve into a presidential republic? This is the subject addressed by William G. Andrews.

In the brief introductory section of the book, Andrews demonstrates that all the framers of the constitution except De Gaulle thought they were creating a parliamentary regime. Most of the work examines how the system evolved in certain critical areas, for example, the relative importance of the cabinet versus the council of ministers, the selection of executive personnel, the lawmaking powers of parliament and the executive. This section is narrow and formalistic; the author describes how the regime evolved, but not why.

In a thirty-page conclusion, Andrews goes to the opposite extreme. In a chapter on "The Transformation of French Society," he asserts that France modernized between 1944 and 1962. Then, in "Elements for a Constitutional Theory," he outlines a general thesis that is purported to explain the relationship between changes in French society and changes in the political system. He develops two ideal types, "cohesive society" and "fragmented society," and argues that parliamentary regimes flourish

in the latter, presidential regimes in the former. Thus, the explanation for the advent of the presidentialist regime of the Fifth Republic is the modernization of France. The most charitable comment one can make is that the interpretation is simplistic and banal. The author exhibits no familiarity with the abundant theoretical literature devoted to this problem, nor does he refer to historical factors like the Algerian War, which certainly had an impact on the development of French political institutions.

At the end the author, anxious to show that the nature of society, not just the constitution, determines how a political system works, attempts a comparative approach. Unfortunately, he seems to have little grasp of the realities of English, German, and French societies. He characterizes Ramsay MacDonald's National Government as Lib-Lab, states that politics in England after 1931 were less ideological than before, defines the Weimar Constitution as presidential without qualification, and claims, quite wrongly, that the French Communist party received 30.5 percent of the vote in 1946. Moreover, he thinks that France is more fragmented ethnically than England or Germany, that minority groups comprise 35 to 40 percent of its population, including ten to twelve million Occitans! To assert that "After MacMahon presidents were ceremonial only, and premiers were weak and transient" (p. 226) is hardly a new or nuanced view of the Third Republic. One must add that the literary quality of the work and of the translations leaves much to be desired; diction ranges from generally pedantic to occasionally purple: "The apocalyptic atmosphere of that frenetic summer gave their tocsin special resonance" (p. 1). In short, this study provides valuable documentation about the evolution of the Fifth Republic, but little explanation.

STEVEN PHILIP KRAMER
University of New Mexico

GUSTAV HENNINGSSEN. *The Witches' Advocate: Basque Witchcraft and the Spanish Inquisition (1609-1614)*. (Basque Series.) Reno: University of Nevada Press. 1980. Pp. xxxii, 607. \$24.00.

Between 1609 and 1614 the Basque province of Navarra suffered one of the largest witchcraft panics in all of European history. Nearly two thousand persons, (including 1,384 children below the age of fourteen) stood formally accused before the Inquisition, and five thousand others were said to have been seen at the witches' sabbath. Owing to the traditional Spanish policy of extreme caution with respect to witchcraft, however, and owing to the energetic and skeptical intervention of one of the

junior inquisitors, Alonso de Salazar Frias, this mammoth panic did not result in thousands or even hundreds of executions. Instead, after six persons had been sent to the stake in 1610, arrests, trials, and confessions continued to mount, but executions ground to a halt. Until recently most of our knowledge of this Basque witch hunt rested on the account given in 1612 by Pierre de Lancre, one of the judges in a simultaneous series of French witchcraft trials, and on the account of H. C. Lea. In this book, Gustav Henningsen's major achievement is to show, with meticulous and voluminous documentation, just how the Spanish trials worked, how the fear of witchcraft was spread (with interesting reflections on dream epidemics), and by what means the Spanish Inquisition restored calm to the panic-stricken countryside. Henningsen is a Danish folklorist, thoroughly acquainted with the best anthropological writings on witchcraft, but it is more important to note that he has spent years in Spanish archives and has learned to put the remarkable records of the Spanish Inquisition to historical use, not just for a history of the Inquisition itself but for the history of Spain and of witchcraft generally. His story also has a hero, Alonso de Salazar Frias, whose investigation into Basque witchcraft set new standards for scrupulous, rational, empirical, even experimental, inquiry. By 1611 Salazar had come to doubt whether there was any witch cult at all, and his reports were ultimately so persuasive that the *Suprema* in Madrid adopted in 1614 a new set of strict regulations that put an end to inquisitorial witch hunting in Spain.

Henningsen's book is so firmly rooted in the inquisitorial records that he tells us little about the relation of witchcraft ideas and fears to the local community, but in exchange we get what is surely the most thorough study of any major outbreak of witch panic in Europe. Students of witchcraft have tended in recent years to place so much emphasis on the history of popular culture and of popular religion that they will have to pause to digest Henningsen's important conclusions, for the story he tells is ultimately one of procedure. It was a breakdown of inquisitorial procedure that unleashed the witch craze of 1609-14, and it was a restored sense of procedure that brought it to an end.

H. C. ERIK MIDELFORT
University of Virginia

R. DAN RICHARDSON. *Comintern Army: The International Brigades and the Spanish Civil War*. Lexington: University of Kentucky Press. 1982. Pp. 232. \$19.50.

The role of the International Brigades in the Spanish Civil War has awaited its historian since the

pioneering work of Verle B. Johnston (1967) and the less noteworthy efforts of Vincent Brome (1965) and Jacques Delperrié de Bayac (1968). R. Dan Richardson's scholarly account will probably not discourage one more, since it is short and does not attempt a detailed study of the brigades in action. It also contains some debatable ideas. He tells us at the outset (p. 3) that each side in the war "sought . . . the destruction of the Spanish Republic." He considers that the elections of February 1936 gave only a narrow victory to the leftist coalition. This is misleading: in the 1936 elections, the left won 256 seats against 143 for the right and 54 for the center, including the Basque nationalists. The left republicans who formed the government may have had only 157 seats, but against the right they had the support of the 85 Socialists and 14 Communists. He also downplays the Anti-Comintern Pact, as if to present this offensive military alliance as a threat to the peace and security of the world was merely Soviet propaganda (p. 7).

These considerations aside, Richardson ably demonstrates that the brigades would never have come into existence had it not been for the decision of the Soviet Union to intervene (through the Comintern), for in the opening weeks the number of foreign volunteers was negligible. But the creation of the brigades placed the Communists in a predicament: how to persuade the world that the brigades were not a Communist organization, while at the same time take credit for their discipline, unity, and strength. Richardson also shows that, when later the brigades were assimilated into the Republican army and Hispanicized (the Garibaldi fell to only 20 percent Italian), the Comintern retained its control.

Resistance to its discipline was inevitable. Volunteers weaned on antimilitarism arrived expecting to find an egalitarian army. Instead they found an elitist, puritanical one. Hard drinking, whoring, and obscenity, the soldier's rights, were frowned upon by commissars they quickly came to scorn as a "prissy clique" of "comic czars" (p. 129). Numbers declined, the result both of the high casualty rate (75 percent in many battalions) and of the decrease in the reservoir of new recruits, which was exhausted by spring 1937. Tales of disillusionment and terror kept further recruitment to a minimum. The brigades' press demanded, like their fascist enemy, "absolute and blind obedience" (p. 134), and the smallest act of indiscipline might be regarded as Trotsky-inspired. Home leave was denied, out of fear that none would ever return. Desertion took its place: by March 1937, among the French volunteers alone, no fewer than four hundred had been evacuated by their consul in Valencia (p. 170). Delperrié's view that the reports of terror were exaggerated is not borne out by Richardson: when, for example, the British battalion commander Fred Copeman

went to brigade headquarters to obtain the release of his battalion commissar Wally Tapsell, he told his machine-gun commander to bring his guns and rescue him if he did not return within two hours. (That did not save Tapsell.)

Richardson's conclusion is that the brigade commissariat controlled the press, the police, the hospitals, the prisons, and the officer and commissar training schools of the brigades, but not all this is demonstrated in the book. There are some excellent thumbnail sketches of leading figures in the brigades, but there are some notable lacunae. There is no mention of Leonid Navitsch or Henri Dupré. There is no new information regarding Gaston Delasalle, and the cause of the dissolution of the XIII Brigade after July 1937 is still a mystery. There is no oral history, despite the opportunities. There are three dozen minor errors that were missed in proof, which is disquieting in a university press. André Simone, Auguste Lecoeur, Julián Zugazagoitia, André Malraux, Segismundo Casado, and Manuel Uribarri are all misspelled. "A comrade Zyromski" (p. 153) is in fact the SFIO's left-wing leader Jean Zyromski. Ernest Bevin is confused with Aneurin Bevan (p. 157). John Cornford is introduced three times. Some sentences are virtually repeated (pp. 18–22, 140–46, 140–48). The trains for Spain left not from the nonexistent Quai d'Orsay station (p. 45) but from Austerlitz. A misunderstanding of the French terms *responsable* and *salut* (Italian, *salute*) results in two meaningless comments (pp. 42, 190). And it was Colonel Lunn's international patrols, rather than the French border patrols (p. 46), that the volunteers had to elude, at least from April 20, 1937.

This book is nonetheless a distinguished contribution. It is the first to make a systematic study of the primary sources, ranging from the early mimeographed newssheets *Vers la liberté, Noi Passeremo*, and *Dombrowski* to the centrally edited *Volunteer for Liberty* (in its multiple editions) and *Le Soldat de la République*. If there is a point on which I disagree with the author, it is his tendency (pp. 30, 33) to overlook a prime motive for any volunteer to take part: fear and hatred of fascism. What else should the committed antifascist have done? Surely he could not wait for the bourgeoisie to resist. Romain Rolland is simply the best known among those who turned from communism out of their loathing for Stalinism only to return to it in desperation when faced with the horror on the other side.

DAVID WINGEATE PIKE
American College in Paris

JAN BRUNIUS. *Bondebygd i förändring: Bebyggelse och befolkning i västra Närke ca. 1300–1600* [A Rural Area in Transition: Settlement and Population in Western Närke, ca. 1300–1600]. Summary in English.

(Det Nordiska Ödegårdsprojektet, number 9; Bibliotheca Historica Lundensis, number 45.) Lund: CWK Gleerup. 1980. Pp. 205.

With a provocative title emphasizing changing settlement and population patterns in central Sweden, this book will startle both medieval and early modern historians. Accustomed to a late medieval agrarian crisis, characterized by famine, epidemic, and deserted farmsteads elsewhere in Europe, the medievalist will learn that such a crisis cannot be shown from Swedish data. On the other hand, the early modern historian, to whom precapitalist European society was stagnant, will have to come to terms with an economic expansion in Sweden during the sixteenth century.

Until the 1960s Scandinavian historians assumed that the late medieval agrarian crisis in Europe was universal and tried to explain by demographic and economic factors the ubiquitous deserted farmsteads found in the North. By the middle of the decade, however, they realized that Scandinavia did not fit the European pattern and that much work was needed before an alternative model could be presented. The result was the ambitious Nordic Research Project on Deserted Farms started in 1972 of which a dozen volumes have been or soon will be published. Microcosmic in scope, these studies select areas with geographic and cultural variations as well as sufficient sources to permit an uninterrupted analysis from late medieval through early modern times, thus permitting the testing of several hypotheses.

Eleven parishes covering 1,340 square kilometers in the western and central parts of Närke in central Sweden meet these criteria in Jan Brunius's investigation. While his sources are varied, consisting of diplomas, accounts, cadasters, tithe-, tax-, and prayer-rolls, his attention is limited to settlement and population, and only the former in the late Middle Ages (chap. 2). The years 1300–1500 exhibit a rate of deserted farms (5–11 percent of the settlement at 1550) lower than Norway and other parts of Sweden examined in the project. Since the intensity of abandonment was greatest during the late fifteenth century, it was unconnected with the Black Death. A study of the rents collected from the farms also confirms relative stability during the late medieval period.

Turning to yearly crown cadasters from the second half of the sixteenth century, Brunius demonstrates (chap. 3) a remarkable increase in settlements averaging 17 percent for the whole area but mounting to 95 percent in some of the forest parishes. The author is more concerned about explaining these variations than the larger issue of the expansion itself, mainly because it gives him opportunity to test a particular methodological problem: whether the

division of farms among several families resulted in more underrepresentation of units in certain parishes than in others.

The most interesting part of the book is chapter four where the author deals with the complicated relationship between settlement and population, taking into account the results of recent demographic research. Two unique sources, a list of names for intercessory prayers from the late fifteenth century and the ransom of the fortress Älvsborg from the early seventeenth century, used together with the material from the crown cadasters, enable him to examine the influence of migration and mortality on the population in the intervening period. His conclusions indicate that, while mortality and migration were not excessive during the sixteenth century, the hardships incurred from higher taxes in the early seventeenth century resulted in increased migration by farmers seeking poorer soils and lower rates.

This is sound and careful scholarship that will whet the appetite for the comprehensive conclusions in the final volume of the project. The book is provided with a good English summary and has a few maps, suitable, however, only for Swedish readers.

JENNY M. JOCHENS
Towson State University

BRITT-MARIE LUNDBÄCK. *En industri kommer till stan: Hudiksvall och trävaruindustrin, 1855–1876* [An Industry Comes to Town: Hudiksvall and the Timber and Lumber Industry]. (Acta Universitatis Upsaliensis, Studia Historica Upsaliensia, number 123.) Uppsala: University of Uppsala; distributed by Almqvist and Wiksell International, Stockholm. 1982. Pp. 194. 11 KR.

This book is an interesting example of the growing fields of social and urban history. The expansion of the timber industry was important in the breakthrough of industrialization in Sweden. In the late 1850s this industry came to Hudiksvall, a coastal town in northern Sweden. This dissertation investigates the economic, social, political, and demographic changes that followed its establishment and how the old burgher town reacted to these changes.

The political reform of 1862 transformed the city government. An elite of wealthy merchants and industrialists came into power. In 1868 the same elite took over the management of the sawmill. Their management of the city did not go unopposed. The opposition was led by fishermen, who had been forced to compete in the labor market during the winter with workers who had moved into the town. The mounting dissatisfaction reached its peak in the winter of 1869, when the city council broke with the traditional distribution of communal work. On March 4 fishermen and native workers

rioted against the elite. Britt-Marie Lundbäck shows how this event was connected with the struggle for power in the town. This is the most valuable part of her study. The protest changed nothing. Instead, the rebellion marked the break between the old town, based on trade, crafts, and fishing, and the new industrial town.

In treating the labor market and employment, the author uses population censuses and the church records on which they are based. She could not check her results with complementary sources, however, as there are no sawmill payrolls or similar lists left. The sawmills had many seasonal and migratory workers and others not permanently employed and not registered in the church records. This made the summer population much bigger than the winter population. Not even all of the winter population, however, was registered. Some were living there unregistered, and some were registered in the neighboring parish. In the 1870s, says the author, the population can be estimated as being 30 percent larger than registered. Thus, the proportion of economically active population and the distribution of professions calculated by her can be questioned.

The number of economically active married women was limited but cannot be estimated since with few exceptions they are listed as wives. In reality, Swedish church records do not state profession but rather rank or estate. The number of working-class women living with their parents and listed as daughters, who actually worked, must have been much greater than the author's calculation. Many of them were probably employed at the sawmill or in the unloading of ballast and the loading of planks of small dimensions. The female harbor work that was common at loading places in northern Sweden is not discussed by the author.

Summing up, the book gives a vivid picture of the contradictions in the town at the breakthrough of industrialization.

LASSE CORNELL
University of Göteborg

JAMES CAVALLIE. *De höga officerarna: Studier i den svenska militära hierarkien under 1600-talets senare del*. [High-Ranking Officers: Studies on the Swedish Military Hierarchy in the Second Half of the Seventeenth Century]. Summary in German. (Militärhögskolan, Militärhistoriska Avdelningen, Militärhistoriska Studier, number 4.) Stockholm: Militärhistoriska Förlaget. 1981. Pp. 224. 35 KR.

Reading this book is rather like viewing Yosemite with the aid of computer enhanced infrared pictures. Actually James Cavallie's technology and methodology are not very formidable. They are, in fact, simple nose count and correlation stuff. Like the products of our most advanced sciences, howev-

er, the effect is both fascinating and frustrating. We get an overview and here and there some penetration that might otherwise have been denied us. But Yosemite it ain't! Cavallie gives us some interesting (but not surprising) statistics about 258 army and navy officers who held the rank of major, ship captain, or higher in the Swedish military in 1654, 1672, or 1699. He even identifies them by name—again and again—as he drops them into the proper boxes. But they might as well be red, blue, yellow, and brown sticks for all we learn about them as people or even officers.

Cavallie is interested in the social and national origins and mobility of his 258 subjects, the men who commanded the Swedish armed forces during the second half of the seventeenth century, during the flood tide of Swedish power. First and foremost, he looks at the status, noble or nonnoble, of officers at birth and at a number of points during their careers. He seems pleased to discover that a fair number sprang from nonnoble families. Virtually all, however, and this we knew, were nobles by the time they achieved any sort of rank. (One is reminded of Communist party membership among first ranking officers of the Soviet military.) Nobility (the title, that is) was both reward and prerequisite for advancement in the Swedish military hierarchy—and the more so as the century progressed. Strangely enough, Cavallie states outright (p. 28) that nonnoble origin "obviously presented no hindrance to a successful military career." All his evidence, however, points to the advantages that noble, and particularly high noble, origin conferred for early promotion and achievement of high rank. In later chapters (and numerous tables) the national origins of the high officers are reviewed. Understandably, the number of officers of "foreign" origin (and Cavallie rings all the changes) waxed and then dwindled as the Swedish empire first expanded and then stabilized.

The book is valuable—a useful little supplement to the immense amount of information we have about the Swedish military of that time. Some problems of exact identification of origins are probably not significant. But we do not get to know the high officers. We are told only incidentally, in passing, of the real connections that a few of them used to rise in the hierarchy. We learn little of activities in the decades between the "cross-section" years, 1654, 1672, and 1699. And we learn nothing about competence, incompetence, battle experience, in-service rivalry, nonmilitary service (a few exceptions), or that important factor, character. The German résumé is excellent—but, of course, adds nothing.

HEINZ E. ELLERSIECK
California Institute of Technology

AARRE LÄNTINEN. *Kuninkaan "perintöä ja omaa" (arv och eget): Kameraalhistoriallinen tutkimus Kustaa Vaasan maaomaisuudesta Suomessa vuosina, 1531-1560* [The King's Inheritance and the King's Own: A Fiscal-Historical Study of Gustav Vasa's Landed Property in Finland, 1531-60]. (Studia Historica Jyväskylän, number 21.) Jyväskylä: Jyväskylän Yliopisto. 1981. Pp. xi, 211.

The goal of this study by Aarre Läntinen is to investigate Finland's older history from the point of view of the Finns themselves, for he is under the impression that Finland's history up to 1809 has been analyzed predominantly from the viewpoint of the royal palace in Sweden. Because of the Western cultural context, this viewpoint is different from that prevailing during the period of autonomy (1809-1917), when the goals and measures of Russia were judged solely by Finnish standards.

Läntinen's argument is extremist, for there is a long tradition of research into administrative and social history in Finland. Contrary to what the author says, the emphasis in this kind of study has been on Finland and particularly on settlement, which is the subject of the present study. The starting point has been predominantly Finnish, and views held in Sweden have not been unduly respected.

The author has, however, found an interesting and unstudied subject of investigation in the history of Finland's settlement: the "arv och eget" estates of the sixteenth century. The proportion accounted for by these estates is insignificant in relation to the total number of estates in Finland, which was more than thirty-three thousand in the 1560s. The number of estates studied by Läntinen was 7 in 1530 and 300 in the 1550s. The number decreased rapidly toward the end of the sixteenth century. Four hundred seventy-five estates, that is a good 1 percent of the total number of estates in Finland in the sixteenth century, were, at least temporarily, "arv och eget" estates.

The concept "arv och eget" means "inherited and owned" and is used to refer to land belonging to the king and the royal family. Läntinen divides this property into two categories: "arv" refers to property inherited by Gustavus I Vasa, often by illegally denying the other heirs of the deceased the right to inherit; "eget" stands for the rest of the land owned by the royal family. The categorization is Läntinen's own and to some extent artificial, for in the documents deriving from the sixteenth century "arv och eget" is used as a single concept.

Läntinen investigates every possible detail of the process through which each Finnish estate was acquired by the king. Gustavus Vasa wished to improve his family's status in every possible way and

wanted all the land he could get in order to own more than the other powerful families in Sweden and Finland. In this respect the king can be considered medieval in his methods, regardless of the fact that he created a modern state and developed his country energetically in the various sectors of administration and economy. The majority of the property acquired by the king, more than four thousand nine hundred farm estates, was situated in Sweden. The 475 Finnish estates were a modest minority.

The most valuable element in Läntinen's study is that he has analyzed his sources thoroughly by going over the operating principles of the king. He has studied the history of as many estates as possible in the greatest possible detail and found out to what extent Gustavus Vasa presented himself as the heir of the Sture family, demanding his lawful share of the estate of the deceased, to what extent the issue concerned transferring church property into the hands of the king, and to what extent other procedures were applied. It is particularly illustrative of the character of Gustavus Vasa that people often ventured to placate his anger by surrendering property to him. When Maunu Niilonpoika let Viipuri Castle fall into disrepair during the war against Russia between 1555 and 1557 and was otherwise slovenly in the execution of his duties, the enraged king demanded twenty-one estates from him in compensation.

Läntinen's work is lacking in methodology. He has done a thorough job in studying his material, but the amount of detail in the book puts a strain on the reader's patience. It is a laborious job to find the essential research results from among the plethora of detail. The study as a whole, however, illustrates how thoroughly it is possible to analyze the history of settlement and administration in Finland in the sixteenth century and how much documentary information there is still left from this period.

ANNELI MÄKELÄ
University of Helsinki

TURO MANNINEN. *Vaparaustaistelu, kansalaissota ja kapina: Taistelun luonne valkoisten sotapropagandassa vuonna 1918* [War of Liberation, Civil War, and Revolt: The Nature of the Conflict in White War Propaganda in 1918.] Summary in English. (Studia Historica Jyväskyläensia, number 24.) Jyväskylä: Jyväskylän Yliopisto. 1982. Pp. x, 269.

Turo Manninen's book is a doctoral thesis using the highly formal style and methods of presentation that are now standard with academic historians in Fenno-Scandia. It is a form of presentation that must be virtually unreadable, even for educated laymen, but of course it is not meant to be read by

laymen; it is written by a professional academic for other academics. The aim is to examine the image of the Finnish civil war of January–May 1918 in the propaganda of the victorious Whites. Manninen has analyzed the content of official public statements, of a few private diaries, and of all the extant newspaper editorials, which provide the overwhelming bulk of his sources, to show what kind of vocabulary and what sort of concepts they used in discussing the war. He finds that the basic concept, that socialism was an alien force that presented a criminal challenge to the basic values of civilized society, was already well established in bourgeois minds by 1914. Consequently, these men naturally interpreted the attempted seizure of power by the Finnish socialists as the carrying out of this criminal threat and the White cause as the divinely sanctioned defense of basic moral values. They knew that their Finnish opponents were assisted by Russian soldiers and encouraged and supplied by the Bolshevik regime in Russia. Initially, however, they recognized that this was a civil war in which the role of the Russians was as auxiliaries. Manninen then shows how, parallel with this concept, an alternative view developed, which reversed the relationship and presented the Bolsheviks as the main participants, seeking to uphold Russia's imperialist grip on Finland, with the Finnish Reds as traitors and their willing collaborators. In this view the civil war became a war of independence, a war between Finland and Russia. This interpretation became more attractive as the situation developed, because it provided good justification first for German armed intervention and then for Finland's growing aspirations to intervene in, and eventually annex, Russian Karelia. This led to the paradoxical situation that while the actual level of Russian participation decreased its role in White propaganda steadily increased.

These points are established with scholarly accuracy, a meticulous attention to detail, and on the basis of a wide range of source material. It is a thoroughly professional piece of work. Very occasionally the professionalism slips a little. When Manninen draws a parallel between atrocity stories in the Finnish Civil War and similar stories in the Spanish Civil War of 1936, he fails to note the significant difference that the Spanish atrocities really happened, the Finnish ones were almost wholly invented. Such lapses scarcely matter; the book has certainly extended knowledge about the Finnish Civil War. It would have been even better if Manninen had gone on to show how the facts he has so meticulously presented can be used to deepen our understanding of it. There is a brief English-language summary appended.

ANTHONY F. UPTON
St. Andrews University

A. D. WRIGHT. *The Counter-Reformation: Catholic Europe and the Non-Christian World*. New York: St. Martin's Press. 1982. Pp. 344. \$25.00.

This is a wide-ranging, stimulating, and often insightful book. But it has serious deficiencies. A. D. Wright intends it "for interested readers new to the subject as a guide to the issues involved in any serious study of the Counterreformation" (p. vii). Despite the subtitle, the book is not about the meeting of Christian and non-Christian cultures in Asia and America, though it does bring out the reciprocal influence of events in Europe and missionary activity across the seas. Nor is there much discussion of political efforts to restore Catholicism in Protestant areas. The focus is on the reform of the church in Catholic lands. Wright argues that the Counter Reformation must be evaluated on the basis of its success, up to the end of the eighteenth century, in preserving Catholic orthodoxy and especially in bringing about an intelligent practice of the faith among the common people. He does not enter a final verdict, which must await further research, but he is clearly inclined to a positive judgment. Following H. O. Evennett and J. Delumeau, Wright sees both the Reformation and the Counter Reformation emerging from the broadly based desire for church reform in the later fifteenth century, and he stresses elements common to both movements, such as the heavy involvement of the state in ecclesiastical affairs. A principal theme is the influence during this period of the patristic writers and particularly of Augustine as the theologian of grace and as the model bishop. Wright also discusses topics such as the impact of the Counter Reformation on economic development and on the rise of science.

Some interpretations of the author and some of his facts are certainly open to challenge. But my chief complaint is his failure to appreciate adequately as a standard for evaluating the Counter Reformation its success or failure in adapting the church to the new circumstances of early modern Europe. Wright sees church reform excessively in terms of a return to an earlier norm and not sufficiently in terms of an updating to contemporary circumstances. The spiritualities of Ignatius Loyola and Francis de Sales were largely responses to Christians looking for a way to live their faith actively in this new world. In his emphasis on Augustinian influence, Wright fails to note the contributions of the great Scholastic thinkers, such as Vitoria and Suarez, who among other things helped lay the foundations for an international law suitable to the young sovereign states and the wider world of the sixteenth century.

Despite the citation above, this is not a book for beginners; it presumes considerable familiarity with the period. It is poorly written, with excessive and

often improper use of commas, semicolons, and colons. Sentences are frequently long and complicated. Organization is weak. The development is thematic, and there is a confusing movement from century to century and back again. Nor are there maps or chronological tables to help the reader. The notes point chiefly to archival sources, usually found in Italy. There is a lengthy and valuable bibliographical essay comprising works in the major European languages; it would have been more valuable had more attention been given to organization.

Much reading and research has obviously gone into this book. We should be grateful for the learning and the insights, but I wish the editors had given more assistance with the writing.

ROBERT BIRELEY

Loyola University of Chicago

HARRO HÖPFL. *The Christian Polity of John Calvin*. (Cambridge Studies in the History and Theory of Politics.) New York: Cambridge University Press. 1982. Pp. x, 303. \$44.50.

This book attempts to "uncover the relationship between Calvin's practical experience as a political actor and his political theology" (p. 1), correcting what its author believes to be the unhistorical and even anachronistic analyses of most previous interpreters of Calvin's thought. It is based primarily on acute and sensitive readings of Calvin's writings. The first five chapters place his early writings in strict chronological order, from 1532 to 1543. From them emerge a succession of political ideas advanced by a man writing first as a student of law and rhetoric, then as a summarizer of Reformed theology, then as a fledgling minister in Geneva, then as a minister to refugees in Strasbourg, and finally as new director of Geneva's ecclesiastical establishment. A sixth chapter sketches Calvin's career in Geneva from 1541 to 1564; three subsequent chapters develop an extended analysis of Calvin's mature political thought pieced together from many different writings. A final chapter summarizes and concludes. Two appendixes examine Calvin's conversion and his doctrine of predestination.

Höpfl is superb as an analyst of Calvin's thought. He is very sensitive to the precise nuances of Calvin's language, particularly his use of such key terms as *politia* and *potestas*. He carefully connects Calvin's political ideas to the ecclesiological and moral ideas with which they are so often juxtaposed, sets this complex of ideas within the context of Calvin's theology, and then sets that theology within the intellectual context of the period. These methods make it possible for him to present the most persuasive analyses now available of Calvin's attitude to-

ward different forms of government, his views about the proper functions of government, and his teachings about the obedience due to a government by its subjects. By rooting these ideas firmly within the Genevan context in which they matured, Höpfl also helps to explain why and how they had to be modified when applied to France and other countries after Calvin's death. He even has some fresh observations about the place of predestination in Calvin's theology.

Höpfl is not equally strong, however, as a biographer of Calvin or as a student of Calvin's Geneva. This weakness may derive in part from the extremely concise form of his treatment. But it remains an exaggeration to assert that Calvin had no personal knowledge whatever of princely courts (pp. 31–32, 160); for, even though most of his working career required negotiation with independent city governments, he had been raised at the court of a bishop, had close friends at the royal court of France, and had visited the court of Ferrara. This weakness also derives from Höpfl's failure to consult all the relevant secondary literature. It is foolish, for example, to estimate the population of sixteenth-century Geneva (pp. 57, 136) without consulting the expert demographic studies of Alfred Perrenoud. And it is ignorant to claim that "there seem to be no reliable figures for membership and actual attendance" in the Genevan consistory (p. 269, n. 2), when precise records do in fact survive and were used by E. W. Monter in an important article published in the *Bibliothèque d'Humanisme et Renaissance* (1976).

Fortunately these flaws are minor and quite peripheral to Höpfl's central argument. His book remains a study that must command the attention and respect of every specialist on the Calvinist Reformation.

ROBERT M. KINGDON
University of Wisconsin,
Madison

JAMES O'HIGGINS. *Yves de Vallone: The Making of an Esprit-Fort*. (International Archives of the History of Ideas, number 97.) Boston: Martinus Nijhoff; distributed by Kluwer Boston, Hingham, Mass. 1982. Pp. vi, 248. \$35.00.

Yves de Vallone began his career as a member of the Congregation de France. Secretly converted to Calvinism, he fled to Switzerland and was received into the Calvinist church in March 1697. Ordained as a Calvinist minister, he migrated to Holland three years later. He soon quarreled with the elders of the Calvinist church, too. As a result he moved once more. On this occasion de Vallone's migration took

the form of an internal emigration toward deism. The result was a manuscript *La Religion du Chrétien*—a never published but still remarkable example of early deist thought.

James O'Higgins has reconstructed the biography of de Vallone and the evolution of his thought. In the first place O'Higgins takes the reader on a fascinating scholarly voyage leading first to the discovery of the manuscript in the Austrian National Library and then to the authentication of de Vallone's authorship of it.

O'Higgins recounts de Vallone's biography and then proceeds to an exhaustive analysis of *La Religion du Chrétien*. We are given a systematic and careful analysis of de Vallone's concept of God, the soul, authority, and religion. The work concludes with a scrupulous comparison of de Vallone's system with other deist and atheist works from the same period like those of Curré, Meslier, and the author of the *Examen de la Religion*. As a result the reader is provided with a concise survey of early Enlightenment thought.

De Vallone appears to have been driven toward deism in part by the historical and biblical scholarship of Richard Simon and Pierre Bayle. But more important seems to have been a rationalist determinism that he assimilated from Descartes and later Spinoza. De Vallone's philosophical determinism led him to Jansenism, then to Calvinism, and finally toward a rejection of revealed religion in favor of a God of reason. De Vallone broke with the Calvinist theologians because they could not accept his insistence "that rigorously speaking God does not love man." De Vallone's rejection of a God who loves mankind followed from his conception of God. According to de Vallone a Calvinist God could only love the elect. The Calvinist establishment was embarrassed by this extreme formulation, which smacked more of metaphysics than Scripture. Indeed, de Vallone's harshness, although ostensibly philosophical, makes one suspect that something beyond reason lay behind this definition of the relation between God and humanity.

O'Higgins is obviously biased in favor of Christianity and against the Enlightenment and the French Revolution. This has not prevented him from writing a scrupulous, learned, and lucid account of an early Enlightenment thinker.

De Vallone's evolution from Catholicism to Calvinism to deism throws some light on the birth of Protestantism itself. The latter has been called by contemporary historians a religious or early enlightenment. The historical and critical element in Calvinism as well as its rationalist and determinist flavor prompts this interpretation. Certainly de Vallone's experience moving from Calvinism to deism under the force of the powerful draft of both seventeenth-century rationalism and critical historical scholar-

ship lends this view of the relationship between Protestantism and Enlightenment further support.

HENRY HELLER

University of Manitoba

ETIENNE FRANCOIS. *Koblenz im 18. Jahrhundert: Zur Sozial- und Bevölkerungsstruktur einer deutschen Residenzstadt*. (Veröffentlichungen des Max-Planck-Instituts für Geschichte, number 72.) Göttingen: Vandenhoeck und Ruprecht. 1982. Pp. 220. DM 49.

During the past fifty years some of the truly stimulating books on eighteenth-century German social history have been written by non-Germans, such as Brunschwig's and Rosenberg's books on Prussia, Dreyfus's book on Mainz, Mack Walker's on the so-called home towns, and now Etienne Francois's book on Koblenz. For the greater part of the eighteenth century Koblenz housed the *Residenz* of the archbishops of Trier and therefore was for all practical purposes the capital of the bishopric of Trier. Even by eighteenth-century standards it was a small town with only about seven or eight thousand inhabitants. The small number of inhabitants and good archival material enabled Francois to investigate the demographic development and the social structure of the town in depth and with precision. In spite of a high birth rate, the population of Koblenz would have declined in the course of the eighteenth century had it not been for a steady stream of immigrants. Of course, a very high rate of infant mortality was the major reason for the decline of the population. But to what extent was the generally high rate of mortality conditioned by famine and epidemics? And then, who were the immigrants, where were they from, and what were their occupations? Francois tries to provide answers to these questions on a firm statistical basis.

Analyzing the social structure of Koblenz, he applies three major criteria: (1) occupational structure, (2) house ownership, and (3) rate of literacy and social background of students in the college. Francois's adroit handling of statistical methods is quite imaginative and instructive. Although his three criteria are of a very different nature, they lead to strikingly similar conclusions and reinforce the findings. I must, however, admit that I have kept wondering whether the data on house ownership constitute a sufficiently valid basis, because they cover less than half the population and include only the value of the house proper, not that of adjacent gardens or land. And yet, the conclusions presented are convincing. A small but revealing and fascinating table on page 80 summarizes the conclusions in a few lines. I was particularly intrigued by the correlation between social status and family size.

The second part of the book offers a descriptive

analysis of the various social orders: the upper class, consisting of the imperial knights and the *Amtsadel*, the higher ranks of the civil service and the professions, the clergy, and the rather weak merchant class; the large middle *Bürgertum*, with its mass of craftsmen and the loose group of middle-rank administrators; and finally the lower classes and the truly destitute. Each of these groups is seen in the light of the three major criteria: numerical strength as a social group, social standing as reflected in the value of the house, and cultural accomplishments as expressed by literacy and attendance of the college. It is a happy combination of criteria. The description is often supplemented by maps on the distribution of the various social groups in the city. I was particularly impressed by the discussion of social mobility and a penetrating account of the lower classes. There might be disagreement on minor points: was it really such a good idea to include the group of widows and unmarried women in the lower classes? In short, these chapters present a vivid and rich picture of the social structure and even touch upon the economic life and the social and religious values of a stagnant Catholic *Residenzstadt* in the late eighteenth century. I should think that these descriptive sections are as revealing and instructive as the strictly statistical sections of the book.

According to Francois, three general traits characterize eighteenth-century Koblenz: the stability and conservative nature of its society and the strong impact of the court of the Prince Elector. This characterization may not appear startlingly new. But Francois's study, rich in color and striking in detail, is presented with such freshness and sympathy that it is a pleasure to read this remarkable book. The lucid and attractive presentation is not the least of its virtues.

CLAUS-PETER CLASEN

University of California,
Los Angeles

ISABEL V. HULL. *The Entourage of Kaiser Wilhelm II, 1888–1918*. New York: Cambridge University Press. 1982. Pp. xii, 413. \$39.50.

From the moment that William II became kaiser in 1888, his retinue urged him to assert a full exercise of royal prerogative. William was receptive to such majestic blandishments, since he believed that he was destined for a pre-eminent role not only in Germany but also in all of Europe. Those who hoped that the immature kaiser would provide forceful leadership ultimately were disappointed, however, for William II's erratic temperament and indifference to the substance, as opposed to the spectacle, of monarchy made him ill-suited to play

the autocrat. The kaiser never asserted himself either methodically or with any consistency, and the royal entourage itself thus became the beneficiary of its efforts to create a *persönliches Regiment*.

William II was imperial Germany's albatross, and he has laid his dead weight on Isabel V. Hull as well. In the vast and complex tableau that she has bravely taken for her subject, the kaiser is persistently at the center, and consequently the book's incisiveness depends largely on the acuity of her treatment of this perplexing sovereign. Hull argues monocausally that William is to be understood as a man hopelessly split into a masculine-feminine duality within which he emphasized the former and suppressed the latter. The result was the increasing importance of the military contingent in the retinue, which epitomized the virile image for which the kaiser strove and was sufficiently willful to take advantage of his lassitude. This suggestive theory is not trenchantly developed, especially because what is held to be the feminine side of the kaiser's personality is no more than sketchily analyzed. But even had the author provided a more compelling scrutiny of this alleged duality in the imperial psyche it would not in itself suffice to unravel William's curious personality. Perhaps the kaiser did indeed struggle to suppress the woman who may have lurked beneath all his macho swagger and braggadocio, but there is also the question of his pronounced physical handicap, which Freud rightly noted was related to the pervasive problem of his maternal difficulties. William II plainly suffered from an inferiority complex rooted not in his repressed femininity or withered arm but in his awareness that, for all his theatrical efforts, he was by professional standards not every inch a king or even much of a soldier. It may be that the kaiser is to be understood, as Thomas Kohut argues, as a man in search of sublimative self-objects. Whatever made William what he was, it is certainly to be uncovered only by an exacting review of all the possible ingredients.

Hull proves very insightful when she turns to deal with the aristocratic nexus in which the kaiser allowed himself to be imprisoned during his thirty-year reign. In this bleak and joyless world of *Kastendunkel* Prince Philipp zu Eulenburg occupied the leading place until 1897. While we have already learned much about Eulenburg from John Röhl's masterful work, Hull provides an elaborately documented picture of this unctuous courtier and also presents a really pioneering investigation of some of the other figures who surrounded the throne, men such as Baron Axel von Varnbüler and Prince Max Egon von Fürstenberg, who were once public figures of some notice but who now loom only as murky phantoms. So well has Hull executed her task that one might wish she had cast her net more

widely. William's association with the *Bundesfürsten*, and especially with the Zähringen in Karlsruhe, might profitably have been examined, as might the atavistic clerics and artists who were prominent ornaments of the Wilhelmine court. Some important specific individuals are missing. William II's old tutor Georg Hinzpeter makes no real appearance, although for thirty years after his retirement he was a significant figure at court. While the author provides much valuable information on the kaiser's military satellites, she unfortunately omits any discussion of Adolf von Bülow, an adjutant whom the Empress Frederick as well as Adolf's brother Bernhard thought responsible for William's exaggeratedly militaristic manner. Finally, Hull's analysis of the entourage would be refined by a foray into prosopographical calculation so that one could grasp whether the social texture of the entourage changed during the kaiser's long reign. If Hull's view of the kaiser and his entourage has been more restricted than is perhaps desirable, what nevertheless emerges from her illuminating discussion is a meticulously executed portrait of a group of men who have failed to be accorded their historical due until the appearance of this important and very welcome book.

LAMAR CECIL

Washington and Lee University

JOHN C. G. RÖHL and NICOLAUS SOMBART, editors. *Kaiser Wilhelm II: New Interpretations*. (Corfu Papers, 1979.) New York: Cambridge University Press. 1982. Pp. xiii, 319. \$39.50.

In 1967 John C. G. Röhl published his pioneering and now classic study of high political maneuvering, *Germany without Bismarck: The Crisis of Government in the Second Reich, 1890–1900*, in which he diagnosed a successful campaign on the part of a court party to establish Wilhelm II's "personal rule" over the reins of government. This project was orchestrated by the kaiser's favorite, Philipp zu Eulenburg, in intimate association with the future chancellor Bernhard von Bülow. By 1897, when the government was radically reconstructed with a package of new policies, they were successful, and the process was brought to a formal conclusion three years later, when Bülow graduated from foreign secretary to chancellor. Röhl has since consolidated his claims with some valuable essays on the kaiser's contribution to the immediate origins of the First World War and a major edition of Eulenburg's political correspondence (two of the planned three volumes have so far appeared). Despite this scholarship, however, German historians have been unsure about what to make of his central thesis. There have been no

comparable studies of the German executive for either the 1890s or later. In any case, the prevailing structural-functional approach to the history of the *Kaiserreich* and the general turn toward social history have generated a powerful bias against Röhl's type of high political analysis. His "personal rule" thesis, consequently, remains much cited but rarely properly confronted. It is an unanswered challenge to the established interpretations of the field.

With this collection of papers he has now returned to the fray. They mount the strongest case yet for taking the personal power of Wilhelm II seriously. They include explorations of the kaiser's personality and its contribution to the ups and downs of Anglo-German relations (Thomas A. Kohut, Lamar Cecil, Jonathan Steinberg), more general reflections on his place in foreign policy making (Paul Kennedy), discussions of the court (Wilhelm Deist and Isabel V. Hull), close analyses of Wilhelm's relations with Bülow (Kathy Lerman, Terence F. Cole), and a discussion of his standing with the German public (Elisabeth Fehrenbach). The volume is introduced by Röhl himself, who properly (and ironically) castigates the prevailing orthodoxies of Wilhelmine historiography for closing the door to certain kinds of investigation, gratuitously stigmatized as "personalistic." Having persuasively affirmed the legitimacy of the whole exercise, he then contributes a scintillating character sketch of the kaiser, which is easily the best biographical treatment of the subject. The collection is concluded by Nicolaus Sombart with some wide-ranging "reflexions on Wilhelmine society, sexuality and culture."

Two things stand out from these accounts. First, the kaiser was manifestly incapable of governing. He could not care less about the daily grind of political management and government business and took every opportunity to escape. He was much happier charging around the countryside and sailing his yachts, perfecting his dilettantism, boring his companions, and occasionally (as in the photograph on p. 34) sitting on them. While he succeeded in stamping his influence on one or two specific areas (the fleet, the officer corps, certain areas of diplomacy), his activity was sporadic, unpredictable, and overwhelmingly disruptive. Secondly, with certain exceptions (for example, Deist and Kennedy) the contributors take the kaiser largely on his own terms in the immediate context of the court, the entourage, the dynastic connections, and the relationship with Bülow. It is only by doing so, I would argue, that they can make the "personal rule" thesis stick. As soon as we add the wider dimensions of government, theorized within a well-developed conception of the state-society relationship—what in his earlier book Röhl referred to as "the relationship between the Executive and the Legislature and, on a wider plane, between the State and society" (*Germany*

without Bismarck, p. 10)—the case looks far less compelling. It is in precisely those contributions where Wilhelm's relations with the civil executive are directly discussed (Lerman and Cole) that the limitations of a high political perspective are most glaringly revealed.

One need not, however, accept the "personal rule" thesis to profit from this stimulating and immensely enjoyable volume. It strikes a valuable note in the tiresomely embattled arena of German historiography for a refreshing pluralism of methods and modes. Finally, Röhl's challenge still remains, and at some point someone will have to take it up.

GEOFF ELEY
University of Michigan,
Ann Arbor

KONRAD H. JARAUSCH. *Students, Society, and Politics in Imperial Germany: The Rise of Academic Illiberalism*. Princeton: Princeton University Press. 1982. Pp. xvi, 448. Cloth \$40.00, paper \$16.50.

Two years before Hitler became chancellor, the National Socialists captured the leadership of Germany's major student organization. Konrad H. Jarausch's thesis is that "academic illiberalism"—including national chauvinism, antisemitism, and other types of intolerance—emerged during the Second Empire and formed the environment in which the National Socialists could win the allegiance of large numbers of German university students well before the Third Reich. Although some readers may take issue with this thesis, no one familiar with Central Europe has reason to succumb to the dogma still heard in the United States that education destroys intergroup prejudices. As Jarausch suggests in one of his many invitations to take a comparative view of German students: "The Imperial German experience cautions that the humaneness of humanism in higher education should not be taken for granted, that equality in the pursuit of knowledge can insidiously justify privilege, and that the relationship between liberal education and freedom needs to be rethought by every generation" (p. 425). One of the virtues of Jarausch's study is to have clarified for an English-speaking audience the strength of the opposition to academic illiberalism in German universities and, more broadly, in German society. While many academic organizations were bastions of antisemitism, workers' organizations opposed antisemitism and occasionally exhibited philosemitic tendencies. Jarausch frequently mentions Social Democratic critiques of German higher education and academic illiberalism without neglecting the opponents of illiberalism within the universities themselves.

The statistical data he presents are less likely to excite controversy than his thesis. For example, his statistics on enrollments in higher education show an expansion after 1870 because of increasing numbers of petit-bourgeois students. This serves to confirm what we knew from specialized studies undertaken by German scholars during the Second Reich and the Weimar Republic.

The basic strength of Jarausch's work is that it rests on a thorough review of the sources of the data, the recomputation of many data, and a careful examination of the relationship between society, university enrollments, and government policy. Jarausch has understandably restricted himself to gathering extensive data only on a few universities—Bonn, Berlin, and Marburg. These three are well chosen. He supplements his study of them with much material on other universities. The array of sources includes student periodicals, university archives, and state archives. I find one omission in the evidence puzzling. Jarausch does not tap the large body of fiction dealing with students during the Bismarckian and Wilhelmine periods. I wonder what he would do with Helmut Harringa in Hermann Popert's reform novel of the same name, for Harringa fits easily into neither the liberal nor illiberal camps that Jarausch describes. Of course we should be grateful to Jarausch for having prepared the way for others to take into account this sort of evidence.

Jarausch is a proponent of the desirability of compiling statistical series wherever possible—a procedure often known by the misnomer many of its partisans have given it, "quantitative history." But he rarely lets his tables get out of hand. Almost all of them are discussed lucidly in the text. Unfortunately the book's readability is impaired in other ways: by the syntax and by unnecessary jargon. Some of this jargon originates in contemporary social science ("career-line analysis," p. 134), and some of it comes from infelicitous translations from the German ("this world-political cultivation debate," p. 225 n.). The prose is overburdened also by innumerable quotations from the sources and other historians. Ascertaining the origin of a quotation or statement is often unnecessarily difficult because the references in the footnotes are consolidated. A bibliography listing the works cited in the notes would have been very helpful.

The blemishes in Jarausch's book in no way detract from the full measure of its merits. Anyone working on German universities, professors, or social structure during the period 1871–1918 will want to consult this monograph. So also will scholars interested in the comparative history of higher education.

WALTER STRUVE
City College
City University of New York

GERHARD BESIER. *Krieg—Frieden—Abrüstung: Die Haltung der europäischen und amerikanischen Kirchen zur Frage der deutschen Kriegsschuld, 1914–1933. Ein kirchenhistorischer Beitrag zur Friedensforschung und Friedenserziehung*. Göttingen: Vandenhoeck und Ruprecht. 1982. Pp. 393.

In comparison with the extensive literature available concerning the German churches in the Third Reich there are remarkably few works devoted to the same subject during the Weimar period. This should in itself ensure a welcome for Gerhard Besier's volume. The book is, however, of particular value, since it deals with the whole subject of Christian attitudes on the war guilt issue, a topic that amazingly enough does not seem hitherto to have been studied in-depth. The author's research involved first-hand study of relevant material in Germany, London, Paris, Geneva, Uppsala, Amsterdam, and Brussels; thus, the subject is considered in its ecumenical setting. The volume contains more than a hundred well-chosen illustrations that serve to illuminate the problems under review.

Besier offers lucid guidance through the complicated maze of charges and countercharges and demands and counterdemands as he takes the reader, who cannot but feel bemused or shocked by the evidence of stubborn intransigence and failure in charity, on a tour of successive meetings and conferences. Such contacts and deliberations over a period of fourteen years failed to achieve a really decisive breakthrough in reconciliation. Much of the responsibility for this has to be attributed to the failure on the part of the church leaders, with a few notable exceptions, to adopt an independent line when deemed necessary in light of the Christian gospel. Time and again avowed political interests were afforded pre-eminent consideration. The churches, as the author expresses it, stood continually in danger of becoming *Hilfsorgane* of the foreign ministries of their particular countries. Predominant emphasis was placed on admission of responsibility, full-scale reparation, establishment of factual truth, and respect for national honor. Much that was of the essence of the Christian gospel was widely and patently lacking, especially in the early stages; namely, mercy, forgiveness, and reconciliation in the light of the cross.

The ambitious subtitle given to the book refers to the attitude of the European and American churches to the question of German war guilt. There is but little reference, however, to the Roman Catholic church. The author could scarcely have been expected to have extended his research to the United States. As a result, there is a shortage of American material.

It is open to doubt whether the value of the book is enhanced by the appendage of a lecture delivered by the author to a gathering of German teachers of

religion. This contains a plea for an adequate provision in the curriculum for church history, especially the part played by the church in recent issues of war and peace. It leads to a brief consideration of vexing contemporary problems regarding the above.

Secular historians are not noted for their interest in the role of the churches in the twentieth century, but it is to be hoped that this excellent book will attract wider readership than just that of ecclesiastical historians.

RICHARD GUTTERIDGE
Selwyn College
Cambridge University

EBERHARD SCHANBACHER. *Parlamentarische Wahlen und Wahlssystem in der Weimarer Republik: Wahlgesetzgebung und Wahlreform im Reich und in den Ländern*. (Beiträge zur Geschichte des Parlamentarismus und der Politischen Parteien, number 60). Düsseldorf: Droste. 1982. Pp. 303. DM 68.

Proportional representation received much attention from the first authors seeking to explain the destruction of the Weimar Republic. By allowing a multitude of parties to emerge, by dividing the electorate, and by making majorities impossible in parliament, proportional representation supposedly helped destroy faith in parliament. With such assertions having been made, it is surprising that the electoral system of Weimar has until now not been analyzed. Eberhard Schanbacher's fine, if somewhat technical and old-fashioned, study fills a significant gap. It outlines the origins of proportional representation from the discussions in imperial Germany to its introduction during the 1918–19 revolution. Schanbacher reviews the legislation on vote and seat distribution by the National Assembly and the creation of the electoral law of April 1920. After examining the introduction of proportional representation in the *Länder*, he turns to the reform attempts and court struggles on how parliamentary seats were to be adjudicated in practice. He delineates the positions of the various political parties on electoral reform, especially the allotment of excess votes from area to area, before estimating the significance of proportional representation.

Among the significant findings in this legalistic treatise are the calculations on the actual participation rates in imperial elections (due to the exclusion of females and age restrictions, they were between 10 and 17 percent). That the Social Democratic party, which was also at a disadvantage due to the lack of seat redistribution in the wake of urbanization, favored electoral reform is well known. Schanbacher, however, shows that by the end of the war most other parties had also accepted the need for widespread revamping: "Proportional representation corresponded to general expectations and its

introduction was not considered revolutionary" (p. 48). Anchoring this principle in the constitution also proceeded easily enough. The difficulties came with its application: thirty-five electoral districts were created in which one seat was given per party list on the basis of 60,000 votes; excess votes went to the sixteen regional districts (comprising two or three electoral districts) where again one seat was given per 60,000 votes accumulated, and, finally, all excess votes went to the national level and were again divided according to party lists on the basis of 60,000 votes per seat. Less than 30,000 votes meant no mandate was given, so that at the national level one mandate could vary from 30,000 to 89,999 votes based on the distribution of excess votes. In addition, since parties were considered for a mandate at the national level only if they had attained a seat at the electoral district or regional electoral district level, small parties could be at a substantial disadvantage if they had no regional strongholds (p. 88). The attempts to rectify and to adjust this system engendered many party and court feuds, which Schanbacher traces in-depth. He explores the alternatives ranging from single-member constituencies to ending party lists.

The results of the study are not startling. Schanbacher shows that the claims of electorate exhaustion are false, since participation rates remained very high right through the Weimar Republic. He also illustrates that proportional representation hardly increased the number of parties on the political landscape, since similar trends existed in the *Kaiserreich* and a great number of small groups failed to obtain representation. On the main issue, whether proportional representation helped destroy the Weimar Republic, Schanbacher maintains that all discussion must be speculative. He does not think the electoral system was as significant a factor as, for instance, the presidential powers, but he does conclude that a majority electoral system would have made coalition formation easier and thus helped to thwart National Socialism.

DIETER K. BUSE
Laurentian University

ROBERT WISTRICH. *Who's Who in Nazi Germany*. New York: Macmillan. 1982. Pp. 359. \$17.75.

Because of the nature of Adolf Hitler's regime and especially because of the capture of masses of its records, more details are known about its leading figures than about the other totalitarian societies. Scholars and other students of Nazi history have faced the overwhelming task of assimilating the literature on the topic. Yet Robert Wistrich, a professor at the Hebrew University, claims to have written in this volume the "first comprehensive

Who's Who" of Hitler's Germany (p. 1). The book is comprised of roughly three hundred fifty biographies, ranging in length from a paragraph to several pages, of persons who were prominent in the Third Reich and who the author believes formed, collectively, a "representative cross-section of German society" under Hitler (p. 2).

Wistrich admits the difficulties in deciding which persons to include in the book. He has tried for a balance in choosing the figures he highlights, focusing on leading personalities from the Nazi party, SS and Gestapo, civil service, diplomacy, academia, the arts, and the churches. His most successful contributions are the biographies of German actors, film makers, painters, musicians, and teachers who cooperated with Hitler, such as Veit Harlan, Pascual Jordan, Hans Albers, and Herbert von Karajan. This has been a neglected part of literature on the Third Reich. Wistrich has also included biographies of distinguished scientists and writers who were forced to leave Germany for racial or political reasons. A further balance is provided with entries on higher SS and police officials implicated in the "final solution," who often received lenient postwar sentences, and with entries on significant figures from the German resistance.

One of the weakest parts of this biographical dictionary, at least for this reader, is its entries on Nazi diplomacy. Some key figures have been omitted, such as Ernst Woermann and Count Friedrich von der Schulenburg. And there are some inaccuracies. For instance, Alfred Rosenberg was not "nominally responsible for Nazi Parties in other countries" (p. 258). That position fell to Ernst Bohle, head of the party's *Auslands-Organisation*, who is not mentioned.

According to his own determination of what should be included in the book, and given its modest (for a reference work) length, Wistrich has probably produced as comprehensive a volume as possible. It provides a wealth of data not otherwise available in any one place. The same cannot be said of the bibliography.

DONALD M. MCKALE
Clemson University

RICHARD F. HAMILTON. *Who Voted for Hitler?* Princeton: Princeton University Press. 1982. Pp. xv, 664. Cloth \$50.00, paper \$16.50.

In the brief space available it is impossible to do justice to the empirical and historiographic significance of Richard F. Hamilton's new work. Its findings force us to take a new look at much of what has among most researchers been accepted as the answer to the question "why Hitler?" or "why the Nazis?" Hamilton demonstrates that the theory of primary culpability by the lower middle class, which

most historians have cherished for more than fifty years, is simply not supported by the available empirical evidence. The author's presentation of the evidence for his own revisionist thesis is so convincing that no future researcher working in the history of Nazism (or modern Germany for that matter) will be able to ignore this book.

Hamilton poses three major questions in the course of his analysis: where did voting support for the Nazis in the crucial years 1930-32 come from, why did it come from there, and, in a historiographic examination, why have historians and political scientists persisted in upholding a number of theses for which there is little or no evidence?

The author's answer to the first question, summarized all too briefly, is twofold. First, the Nazis gained most of their support from the Protestant countryside and small towns. Second, Hitler's support in the major cities was less strong, but such support as the NSDAP did receive correlated positively with Protestantism on the one hand, and the socioeconomic status of the voters on the other: the wealthier the urban voting population, the greater the Nazi vote. In contrast, lower-middle-class urban areas provided average or even subaverage (though by no means negligible) support for the Nazis. Hamilton provides evidence for his thesis by an ingeniously detailed and completely convincing sociogeographic analysis of the voting returns for the Reichstag elections from September 1930 to November 1932 in a number of major German cities. His studies of Berlin and Hamburg are most detailed, but the author also demonstrates that his findings hold true for urban areas as different as Munich and Wuppertal.

Hamilton's empirically based methods and findings cannot, it seems to me, be faulted. As he readily admits, he is on considerably more speculative ground in attempting to answer the question as to why urban upper- and middle-class Germans voted for a party that was characterized by utter programmatic opportunism, vulgar antisemitism, and the excesses of stormtrooper rowdies. Not all of Hamilton's answers are new, although they bear repeating. Not only were the Nazis tireless organizers and propagandists, but they cleverly suited the message to the clientele: antisemitic propaganda, for example, was assigned a low priority in all areas of Germany that did not have a strong prewar antisemitic tradition. Hamilton also states that the other parties, particularly those oriented toward middle-class votes, had no viable answers to the Depression, although in this section of the book I found the author's characterizations of the German parties a bit one-dimensional and simplistic. Considerably more original, although hardly conclusive, is an attempt to link the voting patterns with the editorials and news coverage of the Nazis in newspapers likely to be read by middle- and upper-class

German voters. Here the author does demonstrate that a number of major newspapers portrayed the Nazis not as something outside the pale, but as an acceptable political alternative for middle-class votes.

Undoubtedly the most controversial parts of the book will be the historiographic sections. Essentially, Hamilton attributes most historians' adherence to the lower-middle-class (or extremist center) thesis to a sort of unacknowledged and residual Marxism inherited from the transplanted scholars of the Frankfurt School. The issue (and Hamilton's argument) is more sophisticated, of course, but the very fact that the thesis has persisted so long without meaningful evidence demonstrates that historians, too, are not immune to the enticements of socioscientific theology. All the more welcome, then, is a work like the present one that is so firmly grounded in the rigors of empirical research.

DIETRICH ORLOW
Boston University

DENNIS E. SHOWALTER. *Little Man, What Now? Der Stürmer in the Weimar Republic*. Hamden, Conn.: Shoe String Press. 1982. Pp. xvi, 285. \$22.50.

Next to Hitler, Julius Streicher is Germany's most notorious antisemite. Through his Nuremberg weekly, *Der Stürmer*, he dispensed the crudest, most virulent, and most sensationalistic form of Nazi antisemitic propaganda. Despite his notoriety, however, there have been surprisingly few scholarly studies of Streicher or his odious periodical. This new study by Dennis E. Showalter (as well as W. P. Varga's 1981 biography of Streicher) helps remedy this unfortunate oversight.

Showalter examines how Streicher's paper fostered German Judeophobia during the Nazi *Kampfzeit* (1923–33). Brazenly appealing to and deliberately nurturing the hatreds, fears, and envy of its lower-middle-class readers, *Der Stürmer* monotonously portrayed the Jew as schemer, trickster, and disruptive miscreant, and the German as his innocent and outraged victim. The stories, editorials, cartoons, and letters in the paper all tended to fit Jews into three stereotypical roles: as lascivious sex offenders who sought the racial pollution of German women and their offspring; as dishonest businessmen who constantly cheated their gentile customers; and as arrogant "bad neighbors" who held their gentile neighbors in contempt and treated them accordingly.

Although it is not as clearly stated as it might be, it seems to be Showalter's thesis that the success of *Der Stürmer* lay in its ability to translate abstract antisemitism and vague lower-middle-class social anxieties into concrete, everyday terms with which

the "little men" of Nuremberg were familiar and with which they could identify. Streicher's genius consisted of exploiting numerous local petty scandals, innocuous everyday incidents, and mundane grievances: a customer shortchanged by a local merchant, a woman insulted or propositioned on a streetcar, a diner who became ill after a restaurant meal, even a dog mistreated by its (Jewish) owner—all were portrayed by *Der Stürmer* as part of a larger pattern of Jewish conspiracy against Germans. By exposing the many petty and seemingly trivial means used by Jews to undermine German society, the paper brought fear of Jews and resentments about their behavior (or their success) down to a daily, neighbor-to-neighbor level. Readers of *Der Stürmer*, in turn, responded by sending Streicher a steady stream of reports, drawn from their personal experiences, as additional evidence of the Jewish conspiracy at work in Nuremberg. Just as Hitler did on a larger level, then, Streicher and *Der Stürmer* seemed to embody and articulate the resentments and fears of the "little men" of Nuremberg; Streicher and his paper posed as their defender against the Jewish-inspired chicanery that, in their eyes, was the bane of their daily existence and the cause of their frustrations.

This narrowly focused monograph provides several useful insights into the most famous outlet for antisemitic propaganda in the Weimar Republic, but offers no major reinterpretations. Although the preface raises several tantalizing questions of a larger nature (the limits of freedom in a democratic society; whether the media of popular culture reflect popular values or create and impose them), these questions remain largely unanswered. The book is factual and informative, integrating *Der Stürmer* well into its broader historical context, but the larger significance of the material presented is never made fully clear.

Showalter's study is meticulously researched: he makes good use of Streicher's personal papers, material from the Nuremberg archives, and all the relevant secondary literature. Would that it were as well written. The style, breezy and colorful, seems self-conscious and at times overly slangy: while most of us are in the habit of drawing analogies to contemporary people and events to enliven our undergraduate lectures, references to James Bond, Archie Bunker, and modern rock concerts strike me as out of place in a monograph of this type. Too often the sentences are ambiguous and enigmatic, leaving the reader to wonder just what it is the author meant. Finally, the more one reflects on the title (borrowed from Hans Fallada's famous Weimar novel), the less fitting it appears.

GARY D. STARK
University of Texas,
Arlington

DIETER OSE. *Entscheidung im Westen, 1944: Der Oberbefehlshaber West und die Abwehr der alliierten Invasion*. (Militärgeschichtliches Forschungsamt, Beiträge zur Militär- und Kriegsgeschichte, number 22.) Stuttgart: Deutsche. 1982. Pp. 363. DM 58.

As an earlier volume in the same series, Klaus Reinhardt's *Die Wende vor Moskau* (1972), did for the Eastern Front, Dieter Ose's book, *Entscheidung im Westen, 1944*, explores the collapse of a strategy. The scene is Normandy and Brittany; the time—after two introductory chapters going back to 1941—is from June 6 to August 20, 1944, D-Day to the breakout from the Falaise pocket. The viewpoint is more operational than strategic: the narrative being pitched, as the subtitle states, to the level of the commander in the theater, the *Oberbefehlshaber West* (*OB West*), rather than to that of Adolf Hitler, who dominated the action nevertheless, but did so from a distance.

The mission of the *OB West* before D-Day was to repel the invasion on the beaches. Thereafter it was to keep the Allied forces bottled up on the Cotentin Peninsula while retaining enough strength to deal with an anticipated second landing north of the Seine River. Two field marshals held the post of *OB West*, Gerd von Rundstedt (from April 1942) to July 3, 1944, and Günther von Kluge to August 17, 1944. Both had to contend, Rundstedt throughout and Kluge until July 17, with an assertive subordinate, Field Marshal Erwin Rommel, the commander of Army Group B. Rundstedt and Rommel have already been subjected, singly and jointly, to about as much scrutiny as the available evidence will sustain; consequently, for the reader, and it appears safe to say for the author as well, the central figure is Kluge.

In the interest of historical accuracy, Kluge is undoubtedly due a reappraisal. For thirty-some years, he has been judged, mainly on the testimony of Günter Blumentritt, the chief of staff he inherited from Rundstedt, to have been at first grossly overconfident and then, after the July 20 attempt on Hitler's life, so fearful of the consequences of his implication in the conspiracy as to be unable to keep his mind on the war. These premises, the author demonstrates, are irrelevant to an understanding of both the battle, which was as well fought as the circumstances allowed, and of Kluge as *OB West*, who himself acted throughout on the admonition he once gave the troops: "die decently."

Painstaking research and attention to detail are the distinguishing characteristics of the books in this series. Although *Entscheidung im Westen* is not an exception, the effect is less impressive because the events and material have already been thoroughly discussed elsewhere, notably, by Gordon A. Harrison in *Cross Channel Attack* (1951) and Martin

Blumenson in *Breakout and Pursuit* (1961). Moreover, the author construes his subject so narrowly as to all but exclude the enemy and considerations on the German side beyond the strict purview of the *OB West*; for instance, the fixed idea of a second landing that persisted well into August. Yet the text, documentation, maps, statistical compilations, and order of battle information will make *Entscheidung im Westen* the standard reference work for the German operations in the Normandy campaign.

EARL F. ZIEMKE

University of Georgia

ALBERTO GROHMANN. *Città e territorio tra medioevo ed età moderna: Perugia, secc. XIII–XVI*. Volume 1, *La città*; volume 2, *Il territorio*. Perugia: Volumnia. 1981. Pp. 580; 585–1178. L. 70,000.

This study by Alberto Grohmann represents a monumental exploration of the socioeconomic and demographic history of the Umbrian city of Perugia and its rural territory during the late Middle Ages and the Renaissance. A thorough, fundamental, and seemingly authoritative piece of research, it is both fascinating and extremely frustrating. It is a work far more likely to be consulted than read.

For convenience the book is divided into two volumes, one devoted to the city and the other to its territory; but at the outset the author rightly insists on the intimate and symbiotic relationship between the city and its territory (*contado*).

This work assumes that the reader is familiar with the general course of Perugian political and diplomatic history. Its nineteen chapters and one-hundred-seventy page appendix of Perugian families (as revealed in fifteenth-century *catasti* or tax registers) include a wealth of detailed information on such matters as urban planning, topography, and building construction; agricultural production; the numbers, occupations, income, economic holdings, and places of residence of various types of Perugians; the patrimonial holdings of urban religious bodies—churches, convents, monasteries, and hospitals—in the fourteenth and fifteenth centuries, and, when known, the numbers of religious in each; and lists with the locations and, when available, tax assessments of hundreds of rural religious entities. Pages 915–1002 contain a detailed "Repertorio" of Perugian villas and castles.

That a final documentary appendix of 139 pages follows a five-page conclusion is indicative of both the strengths and the shortcomings of this work. Put succinctly, there is far too little discussion, analysis, and synthesis, compared to the massive sections devoted to the presentation of archival documentation in transcription, summary, or tabular form. Volume 2, chapter 1, for example, treating the

origin of the *contado* and its subsequent modifications, contains about four pages of text and almost one hundred twenty pages listing various villas and castles. Volume 2, chapter 5 examines the history and demographic structure of the *contado* in about one hundred pages of tables and fewer than twenty pages of discursive analysis (some of which include lengthy quotations from source documents).

This is not to suggest that careful searching will not reveal numerous valuable insights and suggestions. The introduction (pp. 5–16) is devoted primarily to a useful historiographic survey of recent studies in medieval Italian urban history. Volume 1, chapter 2 discloses a “conspicuous investment of noble capital in building” in the first half of the fifteenth century. Volume 1, chapter 6 includes interesting observations concerning the roles played by marginal members of Perugian society—the urban poor, Jews, and prostitutes. (Here I must interject the hope that it is only in this review copy that pages 272–73, 276–77, 280–81, and 284–85 are blank—pages that include the beginning of volume 1, chapter 6.) It was worthwhile, for example, to learn in volume 1, chapter 8 that in the second half of the fifteenth century, as Perugia’s population increased, the holdings of nunneries augmented in value as families sent more of their female offspring into convents in an attempt to avoid fragmenting their patrimonies. For those interested in the long-term demographic trends of the city of Perugia, the opening pages of volume 1, chapter 3 contain valuable generalizations, based especially on the author’s utilization of the tax assessment registers of 1285, 1444, and 1493.

The very diligence, perception, and industry of Alberto Grohmann make me hope that he himself will add to the insights offered here and will favor us with a lengthy, rich, discursive analysis based on the wealth of material in these splendid volumes.

WILLIAM M. BOWSKY
University of California,
Davis

CARLO PONI. *Fossi e cavedagne benedicon le campagne*. (Saggi, number 217.) Bologna: Il Mulino. 1982. Pp. 365. L. 25,000.

This volume by Carlo Poni brings together six essays published over the past twenty years. One appeared in a much less rich English-language version as “Family and ‘Podere’ in Emilia Romagna” (*The Journal of Italian History*, Autumn 1978); the others are collected here in essentially the same form in which they originally appeared, some in very hard-to-find journals. Beyond the convenience of bringing scattered pieces together, and the very informative yet

still incomplete revisions of the “Family and ‘Podere’” article, the book merits attention for the profound lesson it offers—not one without its humbling aspects, I wish to add—on how to combine meticulous scholarship with a passionate commitment to greater social justice.

Poni is a Marxist who works within the theoretical formulations set forth by Emilio Sereni for the study of Italian agricultural history. But theory seems to vanish as the reader wades through page after page of minute detail and documentation on technical matters such as the widths of drainage ditches and the months in which they were dug on this or that side of the Po River. The copious footnotes, careful sketches, and astute use of everything from travelers’ accounts to records of greedy monks evicting tenants with families too small to farm their plots with maximum efficiency, variously may inspire or terrorize eager graduate students searching for a thesis topic and wondering what still needs to be done. Then somewhat abruptly, yet quietly and convincingly, Poni points out to his readers how widths of ditches explain why landlords’ exploitative practices took the forms they did in Emilia-Romagna, an area of Italy where “feudal” and sharecropping systems early gave way to agricultural capitalism and wage labor—and where the Communist party is strong.

Other essays examine plows and hydraulic systems (in the context of a now ancient and evidently youthful rebuttal by Poni to Sereni’s partially critical review of his first book), late sixteenth-century sowing machines in Bologna, the doomed reform programs of L. A. Muratori in eighteenth-century Modena, and the equally hopeless schemes of progressive magnates such as Carlo Berti Pichat on the eve of the revolutions of 1848.

The overall impact of such methodological diversity—literary exegesis, quantification, analysis of family portraits, and hands-on evaluation of technology—brought to bear on seemingly minute topics that suddenly explode with wider significance may differ with the reader. Some may be disappointed over the absence of a grand summary that tells us in a few pages the history of agriculture in early modern Emilia-Romagna. Others may applaud Poni’s decision to leave such generalization as it stood with Sereni and devote two decades of imaginative research strategy to the mundane task of figuring out exactly why things came out as they did. If nothing else, American scholars who peruse the footnotes will know that to get at many of the important questions in Italian history it is essential to abandon the confines of Rome or Florence and look into the provincial and local archives.

RUDOLPH M. BELL
Rutgers University,
New Brunswick

A. ENZO BALDINI. *Puntigli spagnoleschi e intrighi politici nella Roma di Clemente VIII: Girolamo Frachetta e la sua relazione del 1603 sui cardinali*. (Quaderni dell'Istituto di Storia Moderna e Contemporanea dell'Università di Genova.) Milan: Franco Angeli. 1981. Pp. 167. L. 7,000.

After centuries of neglect of what have been called the "forgotten centuries" of Italian history, we are finally turning our attention to the study of the Italian peninsula between the mid-sixteenth and the mid-eighteenth centuries. The work in this area tends to be patchy and appears in the form of detailed monographs. *Puntigli spagnoleschi e intrighi politici nella Roma di Clemente VIII* by A. Enzo Baldini falls within this tradition. It centers around the disgrace of a client of the Spanish monarchy, Girolamo Frachetta, author of numerous occasional political works, who in 1603 was forced to flee from Rome. His crime had been to write an honest but critical account of the Sacred College for the information of the new Spanish ambassador to the Curia, the Marquess of Villena. This was the *Relazione per il marchese di Villena*. The work, destined only for the eyes of Villena, unfortunately fell into the wrong hands and was soon the subject of gossip and scandal throughout the Holy City. Although Frachetta disowned the document, his authorship of it was widely known. Clement VIII, a determined defender of papal rights, took particular exception to two statements within it: the suggestions that papal elections were determined by temporal rather than spiritual intervention and that the Spanish monarchy had succeeded to the role of the Holy Roman Empire.

This work, of impeccable scholarship, is divided into three parts. The first, based on the famous "avisi" from Rome—perhaps the first examples of modern popular journalism—and diplomatic dispatches, describes the numerous diplomatic incidents provoked by the arrogant Villena during his first few months of residence at the papal Curia. It is a masterly revelation of Spanish attitudes toward the Italians, who were frequently treated with boundless contempt even at this critical period when Spanish power in Italy was beginning to wane. The second part of the book deals with Frachetta, his life, the fortunes of his *Relazione*, and the impact that this had on him. The *Relazione* itself is published, together with a number of other related documents, in the third part. As an example of critical thinking and as a demonstration of scholarly historical method, this book cannot be faulted. As an exponent of the "reason of state" philosophy, Frachetta, who is most famed for his *Seminario de' governi di stato et di guerre* (which enjoyed a notable success throughout the seventeenth century), is not without importance in the history of seventeenth-century political theory,

and it is interesting to have new light thrown upon his career. Equally, it is valuable to have confirmation of how seriously Clement VIII took his assertions of Rome's political independence. There are incidental insights into the Baroque civilization of Rome, with its heavy emphasis on prestige, ceremonies, and etiquette. Nevertheless, we will need many more such monographs before any clear picture emerges of the nature of seventeenth-century Italy, the seventeenth-century papacy, Spanish imperialism in Italy, and Baroque culture in Rome.

JUDITH HOOK

University of Aberdeen

COSIMO SEMERARO. *Restaurazione chiesa e società: La "seconda ricupera" e la rinascita degli ordini religiosi nello Stato pontificio, Marche e Legazioni, 1815–1823*. Foreword by LAJOS PÁSZTOR. (Centro Studi Don Bosco, Studi Storici, number 9.) Rome: LAS. 1982. Pp. 503. L. 30,000.

It is surprising, given the theocratic nature of the Papal States, that studies of the political and economic aspects of the restoration there far outnumber those on its religious aspects. Semeraro's work is a welcome addition to the latter. It provides the first serious investigation of the papal attempt to revive the religious orders in the "*Seconda Ricupera*," the two-thirds of the state regained at the Congress of Vienna, as opposed to the "*Prima Ricupera*" recovered in 1814.

The prerevolutionary papal state had been host to a multitude of religious orders, which played a major role not only in its religious life but also in the provision of education and social services. Their suppression by the French regime was regretted by the people and by Pope Pius VII, who on his restoration in 1814 resolved not only to revive them but also to make their revival the occasion for a sweeping renewal of the spiritual life of the church. His dream met with a skeptical reception from the secretary of state, Ercole Cardinal Consalvi, of whom Semeraro is more critical than most historians. He feels that Consalvi, by subordinating the pope's vision to immediate constraints, especially financial, missed the "unique occasion" (p. 255) when such a religious renewal might have been launched. It seems undeniable, however, that the financial crisis was all too real and that any attempt to ignore it in the pursuit of Pius's aims would have come to grief sooner or later. Pius found a more enthusiastic supporter in Giuseppe Sala, secretary of the congregation appointed to revive the orders; Semeraro gives him high marks for ability and energy.

Sala's task, however, proved far more difficult than the pope had foreseen. The property of the

orders had been sold, and the Congress of Vienna ordained that it must be confirmed to its new owners. The ex-religious were scattered, often old and infirm, or diverted into other activities. Many no longer wanted to return to their orders, while those who did preferred a simple restoration of the past to the reforms that Pius sought. The bishops of the Papal States were divided, as was the Curia; and not all shared Pius's enthusiasm for reform. Finally, as Consalvi had warned, the papal treasury was too impoverished to meet the massive expenses that a wholesale revival of the orders would have entailed. In the end, as Sala sadly admitted in his final report, "we have not had the success which we expected" (p. 254). Plans for reform were largely shelved, and only a fraction of the old establishments were restored.

Semeraro has made a useful contribution to our understanding of a neglected but important area. His book is clearly written and based on impressive research in the Vatican and other archives; his conclusions seem generally sound. In this study, Semeraro has concentrated on the process by which Rome reached its decisions. He realizes that further research will be needed to explore the way in which those decisions were implemented in the provinces—and with what consequences. Historians interested in the papal restoration will look forward to his subsequent work in this field.

ALAN J. REINERMAN
Boston College

ROMANO UGOLINI. *Garibaldi: Genesi di un mito*. (Istituto per la Storia del Risorgimento Italiano, Comitato di Roma, Risorgimento Idee e Realtà, Nuova Serie, number 4.) Rome: Ateneo. 1982. Pp. 297. L. 15,000.

Romano Ugolini's volume is a timely guide through the maze of publications marking the one hundredth anniversary of Garibaldi's death. The author, an able representative of Italy's postwar generation of historians, is thoroughly familiar with his subject and the scholarly literature about him. Yet Ugolini has written a book that should appeal to specialists and nonspecialists alike.

The organization of the volume is quite unorthodox, but on the whole it works well. Specialists will certainly profit from reading the lengthy introduction, almost a full chapter, which places Ugolini's contribution in the context of an already vast literature. Nonspecialists will appreciate the judicious balance between analysis and anecdotes that marks the five biographical chapters; but they may regret Ugolini's decision not to go beyond 1846, the year in which Garibaldi returned to his native country after nearly a decade in Latin America.

Aware of the pitfalls of commemorative works,

Ugolini presents a compelling justification for his own. Garibaldi, he points out, is not the kind of historical figure who has been neglected or forgotten and therefore needs to be resurrected in commemorative works. Rather, he is the one Italian national hero whose name has remained a household word in his native country and in others for one hundred years. Garibaldi does not need to be resurrected, rediscovered, or even reinterpreted. The task at hand, Ugolini argues, is to understand why Garibaldi was almost universally popular in his lifetime and why his popularity has endured. Therefore, in the biographical chapters as well as in the introductory analytical essay, Ugolini chooses to emphasize "the myth of Garibaldi"—that is, what Garibaldi meant to nineteenth-century revolutionaries in Italy and in South America—instead of emphasizing specific events in his life and accuracy of historical context. For the same reason, Ugolini concentrates on the pre-1848 period of Garibaldi's life, which previous biographers have regarded merely as preparation for Garibaldi's role in the Italian national revolution.

From Ugolini's perspective, the important ingredients in the making of the Garibaldi myth (the Mazzinian connection, exile, poverty, and guerrilla warfare) were already fully developed at the point when "the hero of two worlds" resolved to leave Montevideo and take part in the movement for Italian unification and independence. Ugolini makes a solid case for this interpretation, all the more so because he builds on the recent, specialized research of Salvatore Candido, Joseph Ridley, Wolfgang Rau, and others.

Ugolini's second thesis is that among the leaders of Italy's national revolution Garibaldi, not Giuseppe Mazzini, made the most lasting impact on other peoples and societies. Unfortunately, Ugolini does not buttress this fairly controversial interpretation with a great deal of evidence. Although he may well be on the right track, he weakens his own case by limiting his coverage of Garibaldi's life to the years 1807–46. For it seems probable that Garibaldi's impact on Central Europe and the Balkans in the 1860s may have been as great as his impact on Uruguay in the 1840s.

To add to an otherwise slim work and perhaps broaden its appeal, Ugolini has added a long appendix (about one-third of the volume) to the introductory essay and the biographical chapters. It consists of Garibaldi's seldom published autobiographical poems and a rare memoir on the war of 1859 in Lombardy by Garibaldi's follower Francesco Carrano. Although none of this material is new, it is relatively hard to come by and therefore particularly welcome as a complement to an interpretive work.

CLARA M. LOVETT
Library of Congress

KARL A. ROIDER, JR. *Austria's Eastern Question, 1700–1790*. Princeton: Princeton University Press. 1982. Pp. 256. \$25.00.

This book in a relatively brief space covers Austrian relations with the Ottoman empire during the eighteenth century. The main theme is the role that Habsburg Ottoman policy played in the wider scheme of the monarchy's foreign affairs. In a difficult strategic position, the Habsburg government had to adjust its policy in any one area to fit into the framework of its general European commitments. Although, by the beginning of the century, the Ottoman empire no longer presented an active danger to Habsburg lands, its increasing weakness threatened Vienna with the possibility that the Ottoman Balkan possessions might fall into Russian hands. As Karl A. Roider, Jr., states, the Habsburg government had three possible policies that it could follow in this situation: first, it could join with Russia and partition Balkan territories; second, it could attempt to annex all or part of the peninsula itself; or, third, it could try to maintain the status quo and thus keep Russia out. The third alternative was usually preferred.

This account skillfully presents the complex picture of eighteenth-century diplomacy and the difficult choices faced by Vienna in meeting, in particular, the ambitious policies of Prussia and Russia. The author recognizes that it was only at the beginning of the century that Austria could have dealt with the Ottoman problems on its own terms; the Habsburg armies won their single clear victory in the war of 1716–18. The decisive change came after 1740, when, following Frederick the Great's invasion of Silesia, Prussia was considered the main enemy. Thereafter, the author writes, "The hatred for Prussia . . . restricted Habsburg policy toward the Ottoman Empire and essentially forced it to support almost anything that the Russians wanted to do" (p. 168). The agreements between Austria and Russia and numerous partition schemes concocted by their leaders were a result of this orientation of policy. By the end of the century the monarchy had gained little. Russia was in control of the northern shores of the Black Sea; Austria had acquired only the province of Bukovina and minor territorial adjustments.

Although this book is intended to deal with the Eastern Question, unlike most books on the subject it does not discuss the movements within the Ottoman empire to attain national or local autonomy. In addition, and perhaps reflecting the Austrian documentation, it has virtually nothing on the people who inhabited the neighboring Ottoman territories nor on the relationship of the Romanian and South Slav people within and without the monarchy. Wenzel von Kaunitz's comments about Moldavia and Wallachia having the "wildest people" (p. 132) and

Maria Theresa's remarks on her dislike of the annexation of areas that were "unhealthy, depopulated, or inhabited by treacherous and ill-intentioned Greeks [Orthodox]" (p. 156) are given, but the Principalities and Serbia, lands directly bordering the monarchy, appear really only as objects of Austrian or Russian conquest. The question of the position of a Catholic power in Orthodox lands is barely touched on. A better background is needed for the author's conclusion that: "Had the Habsburgs annexed the Balkans, they might have become a truly Danubian dynasty, and the presence of a large Orthodox population might have encouraged the liberalization and federalization essential to its survival" (p. 193).

This book, nevertheless, should be read for what it does so well: Habsburg foreign policy is carefully delineated; decisions regarding the Ottoman empire are explained in close relationship with other European issues; and the difficult alternatives facing Austrian statesmen are made clear to the reader.

BARBARA JELAVICH
Indiana University,
Bloomington

ALEXANDER S. MITRAKOS. *France in Greece during World War I: A Study in the Politics of Power*. (East European Monographs, number 101.) Boulder, Colo.: East European Monographs; distributed by Columbia University Press. 1982. Pp. xviii, 258. \$20.00.

French and British troops landed at Salonika, Greece, in October 1915. For the next two years, the Balkan campaign would strain Allied unity. The Allies could not agree on a Balkan offensive. Nor could they agree on which policies to pursue toward neutralist Greece, a country divided between Germanophiles and Ententists.

Alexander S. Mitrakos's book is an attempt to explain France's diplomatic policy toward neutralist Greece from mid-1915 to the spring of 1917. Mitrakos finds French policy unclear and contradictory. Unfortunately, so is Mitrakos's book. For example, Mitrakos insists that at no time during the summer and autumn of 1915 did the Ententist Greek Prime Minister Venizelos promise the Allies that Greece would join the Entente if the Allies were to land troops at Salonika; yet, Mitrakos fails to explain why, a few days after the first French troops arrived at Salonika, Venizelos urged the Greek National Assembly to join the Entente against Bulgaria. Second, the reader is informed that no French official believed that Greece would join the Allies in an attempt to save Serbia in October 1915; yet, a few pages later, Mitrakos observes that the French government was stunned when Greece re-

fused to join the Allies against Bulgaria. Third, Mitrakos insists that Aristide Briand, French prime minister from October 1915 to March 1917, did not want an alliance with Greece because Greece's territorial claims in the Balkans and Asia Minor would have alienated Italy. Yet Mitrakos provides overwhelming evidence that France played the key role in first nurturing the pro-Allied Venizelist movement and then bringing Venizelos to power. Briand certainly wanted Greece to join the Entente in order to supply additional manpower for the Balkan offensive. Fourth, Mitrakos claims that Greek King Constantine followed a policy of neutrality, "scrupulously adhered to" (p. 173), and would have joined the Entente had the French not been so hostile toward him. Mitrakos's discussion of Constantine's policies, however, makes it difficult to believe that Constantine was not a Germanophile. In fact, Mitrakos states that Constantine demonstrated "outright hostility" (p. 179) toward France. Lastly, Mitrakos asserts that France's basic objective in Salonika was to establish a protectorate over Greece so that France would be well positioned to dominate the postwar Balkans and Middle East. Mitrakos presents no convincing evidence to support this claim. Indeed, he tells us that Briand's intentions to impose a protectorate over Greece were "unspoken" (p. 160). They may also have been "unwritten." Mitrakos presents a good deal of evidence from French Foreign Ministry and Naval Archives that demonstrates that French policy was motivated by military concerns, not imperial ones. All of France's hostile actions against Greece sprang from fear that Constantine's Germanophile policies posed a threat to the security of General Sarrail's Allied army in Salonika.

Greater clarity of France's role in Greece would have been realized had the author consulted the wartime sessions of the Chamber's Foreign Affairs and Army committees located at the French National Archives, the Guiot manuscript at the French Naval Archives, the Eastern Army files at the French Military Archives, and the British Cabinet and Foreign Office papers at the Public Record Office.

JAN KARL TANENBAUM
Florida State University

RICHARD C. FRUCHT. *Dunarea Noastra: Romania, the Great Powers, and the Danube Question, 1914–1921*. (East European Monographs, number 113.) Boulder, Colo.: East European Monographs; distributed by Columbia University Press, New York. 1982. Pp. ix, 216.

As the author of this brief study asserts, few questions in Europe's diplomatic history have been as

overlooked as that of the question of internationalization of river traffic on the Danube. Having been accorded authorization to conduct research in Bucharest, Richard C. Frucht apparently proceeded to construct a survey of efforts to establish international regulations of navigation from the Romanian viewpoint. The major focus of the study is upon such efforts during the era of the First World War, and, as Frucht contends, Romania was the leading actor in the drama, which coincided with its emergence as the major power in the Balkans after that war.

The book is divided into eight chapters, which consecutively analyze the background of the question, activities associated with international controls, the impact of the First World War, the immediate postwar era, the relationship between internationalization and the increasing nationalism of the Romanians, the 1920 international conference on the Danube in Paris, the completion of the definitive statute on internationalization, and finally public opinion in Romania aroused by the terms of the statute. Since the thrust of this study is chiefly on Romania's relationship to the internationalization plans, insufficient attention is given to the policies and positions of the other European governments that were involved from 1856 to 1939 in regulating Danube traffic. Perhaps this was the intent of the author, and he should not be criticized for his limited scope. An abiding impression is gained about the permanence of Romania's intransigent nationalism, indubitably the most powerful of any Balkan state during the past century, whether frontiers, minorities, historical claims, or even traffic on a great river is on the agenda. And perhaps that defiant nationalism can be appreciated by those resident in great powers when Romania's independence since 1878 has been almost continuously affected by the interference of those powers in what Romanians consider their internal affairs.

SHERMAN D. SPECTOR
Russell Sage College

JOSEF HARNA *et al.* *Materiály k politickým, hospodářským a sociálním dějinám Československa v letech 1918–1929* [Materials for the Political, Economic, and Social History of Czechoslovakia in the Years 1918–29]. (Sborník k Dějinám 19. a 20. Století, number 7.) Prague: Ústav československých a světových dějin ČSAV, oddělení novověkých československých dějin. 1981. Pp. 582.

The book here under review contains partial and preliminary conclusions for a multivolume "Survey History of Czechoslovakia" (*Přehled dějin Československa*), now being prepared by an "authors' collective" of Czech and Slovak historians.

The two-part first volume of the survey, covering the period from the origins to the year 1848, was published in 1980 and 1982.

When the Communists seized power in Czechoslovakia by the Prague coup of February 1948, Czechoslovak Marxist historiography was only in its infancy. They regarded this as a serious deficiency, since as Marxists they took a determinist view of history and considered historiography not merely as a record of the past but as an important laboratory of active politics. New to power and insecure in holding it, they also wanted to use history to consolidate their government by discrediting the fallen "bourgeois" regime.

In the early 1950s the Communist party took energetic steps to launch a Marxist school of Czechoslovak historiography. Since most established Czech and Slovak historians at the time were not Marxists, their ranks were purged and a crash program to train cadres of historians versed in Marxist ideology was adopted. In a series of conferences, the "theses" of a new Marxist interpretation of Czech and Slovak history were hammered out. Thus, the view that the establishment of Czechoslovakia in 1918 was a result of the Allied victory in World War I was rejected and the country's liberators—Masaryk, Beneš, and Štefánik—were debunked as mere agents of Western imperialism. The emergence of Czechoslovakia was held to be a consequence of the Russian Revolution. Masaryk's belief that the Czechs and Slovaks constituted a single Czechoslovak nation and had a common history, on which the centralist constitution of the First Czechoslovak Republic was based, was discarded. Instead, the Czechs and Slovaks were held to be two distinct peoples with largely separate histories. To serve as a compendium of the Marxist theses of Czechoslovakia's history, an authors' collective prepared a multivolume "Survey of Czechoslovak History" (*Přehled československých dějin*) (1958–60).

During the agitated days of the "Prague Spring" in 1968, many Czech Marxist historians, though few of their Slovak colleagues, joined in the clamor to restore "Masaryk's democracy." Following the Soviet invasion of Czechoslovakia in August 1968 and the ensuing "normalization" of the country, the ideal of Masaryk's democracy, together with that of "socialism with a human face," were denounced as counterrevolutionary and those historians and intellectuals who had espoused them were purged. A rigid Marxian orthodoxy was restored in Czech and Slovak historiography. Its theses are summed up in the new survey history of Czechoslovakia mentioned above. In the volume here under review, a Czech authors' collective has presented "materials for the political, economic and social history" of the Czech provinces of Czechoslovakia (a Slovak authors' collective is preparing analogous materials for Slova-

kia), from the establishment of the republic in 1918 to the outbreak of the world depression in 1929.

While the old survey drew heavily on the basic research of the pre-Marxist Czech and Slovak historians, the materials for the new survey presented in the volume under review appear to be more solidly anchored in Marxist study and analysis of Czech and Slovak history. The First Czechoslovak Republic is still denounced as a "bourgeois" state, but the denunciation is less shrill. The vilification of its founders has been abandoned (Masaryk is treated as a dangerous but worthy enemy of Marxism, Beneš is dismissed as an adroit manipulator, and Štefánik has become a complete nonperson), which would indicate that the Communist party no longer feels threatened by their concept of Czechoslovakia.

VICTOR S. MAMATEY
University of Georgia

ROMAN SZPORLUK. *The Political Thought of Thomas G. Masaryk*. (East European Monographs, number 85.) Boulder, Colo.: East European Monographs; distributed by Columbia University Press, New York. 1981. Pp. 244. \$17.50.

This study, the best in English of Masaryk's complex political thought, surveys "Masaryk's evolution from philosophical and religious cosmopolitanism to a nationalism grounded in history and language" (p. 101) before considering how Masaryk's political ideals conditioned not only his achievements as founder and president of Czechoslovakia but also the goals of the Prague Spring three decades after his death. Among the theses Roman Szporluk advances are three suggested by other scholars but never before so explicitly defined and demonstrated. First, he indicates the great extent to which Masaryk's Christian convictions profoundly influenced his views on social and political questions, especially in his contention that human nature has changed little through history, that "life without faith loses strength and certainty" (p. 179), and that a crisis of civilization wrought primarily by loss of religious faith could not be resolved exclusively by political means. Secondly, in reaction to scholars who have uncritically portrayed Masaryk as a thoroughgoing democrat, Szporluk stresses Masaryk's distrust of the judgment of the masses and desire to have civic-minded experts determine public policy, while reminding his readers how much more devoted Masaryk was to equal opportunity and social welfare than most of his Central European contemporaries. Thirdly, Szporluk emphasizes the remarkable continuity in Masaryk's political thought through fifty years, thus helping lay to rest the mistaken notions that Masaryk was ever a proponent of any idealized Austrian empire or is to be

understood primarily as an opportunistic politician before or after 1914. If Szporluk occasionally exaggerates the extent to which Masaryk based his politics on philosophic conviction as opposed to changing social and political circumstances, it is because he has studied Masaryk's political activity less thoroughly than Masaryk's publications. Moreover, as Szporluk himself acknowledges, many of his views on the unity of Masaryk's political theory and practice must remain tentative because he has not examined the many Masaryk letters and manuscripts in Czechoslovak archives.

For readers unfamiliar with Czech-language sources, Szporluk provides more new information and opinion about Masaryk for the years before 1891 and from 1900 to 1914 than for any other time, even though he says surprisingly little about Masaryk's associations with the women's movement or the Southern Slavs and ignores several of Masaryk's important political essays including *Nynější krise a desorganisace mladočeské strany* (1903) and *Politická situace: poznámky ku poznámkám* (1906). In two chapters about Masaryk from 1914 to 1937, Szporluk offers no new information and few new interpretations while emphasizing the boldness and originality of Masaryk's advocacy of Czechoslovak independence and the tensions between Masaryk's ideals and political reality in the Czechoslovak Republic. Szporluk provides no evidence to support several assertions, as in alleging that during the 1890s Masaryk was more "pro-Austrian" (p. 103) than the Young Czechs and had little understanding of local government. The text is marred by a few factual errors, such as the implication that Albín Bráf was a Young Czech or that independent Czech trade unions did not form until shortly before 1914, and by various typographical mistakes that include misspellings of Szporluk's and this reviewer's last names.

Within the limits he has set, Szporluk has written an informed and thoughtful book whose broad conclusions are not invalidated by any of its shortcomings. Like the writings of Masaryk it so perspicaciously examines, it deserves to find a large audience.

BRUCE M. GARVER
University of Nebraska,
Omaha

KÁLMAN JANICS. *Czechoslovak Policy and the Hungarian Minority, 1945–1948*. Translated and edited by STEPHEN BORSODY. (East European Monographs, number 122; Brooklyn College Studies on Society in Change, number 18; War and Society in East Central Europe, number 9.) New York: Social Science Monographs; distributed by Columbia University Press, New York. 1982. Pp. 240. \$25.00.

It is often forgotten that the end of World War II did not bring about an end to national or racial persecution in Europe. One of the victors, the Soviet Union, came down heavily on a number of Soviet nations, and some of them were deported from their homeland; all of the Allies sanctioned the transfer of the Sudeten Germans from Czechoslovakia as well as a percentage of the Hungarian minority. This book by Kálmán Janics, a Slovak-Hungarian physician living in retirement in Slovakia, purports to tell this latter story. There is indeed a story to be told, but not the way Janics tells it.

A long preface by Stephen Borsody and an introductory essay by Gyula Illyes, a Hungarian writer, precede and set the tone for the essay by Janics. Both let out a *cri de coeur*, as does Janics, of whose essay Illyes writes that it "reveals the hidden feelings and painful needs of millions of his people" (p. 10). That they should wish to do so is understandable, but the cry becomes hollow when they indulge in invective, ideological deprecation, and accusations. As a result it is very difficult to appreciate the full extent of the event and to follow the stages of Czechoslovakia's policy toward the Hungarian minority.

The first chapter entitled "The Accused and the Accuser" is one of the worst in the book. The author seeks to set the historical background; instead, he indulges in a personal interpretation apportioning responsibility and blame as he sees fit. While a historical background is relevant, even necessary, it is clear that this one does not help the reader to understand why "all reprisals against Hungarians were explicitly taken as a punishment of the Hungarian 'Fascist nation' as a whole, not just on the basis of charges of the Hungarian minority's betrayal of Czecho-Slovakia" (p. 47).

Anyone writing on this topic would face a major difficulty in explaining the historical background to the Hungarian minority's plight in postwar Czechoslovakia. The relations between Hungary and Czechoslovakia were not very good in the interwar years, and to a great extent both bear a share of the responsibility for that state of affairs. As Germany became the predominant power in Central Europe, the political leaders of the small nations of that area sought to make the best of an uncertain and difficult situation. The war that followed decided not only who were the victors and the vanquished but also gave the former free license to do what they wished with the latter. It is here that the causality of events and policy became blurred. In the better part of his essay, Janics makes it perfectly clear that this is what happened. It is in the light of this situation that his first chapter is so unsatisfactory.

Czechoslovak policy toward the Sudeten Germans and the Hungarians had been decided by Eduard

Beneš and his Czechoslovak government-in-exile in London. He was on the side of the victors, and he chose what was to be done with the vanquished—and that included not only the Hungarians and the Germans but also those Slovaks whom he declared guilty of treason to his state. Janics makes an effort to outline how Beneš's policy was planned, the difficulties he faced, and the way it was partially carried out. Between November 1946 and February 1947, 42,129 Hungarians were transferred to the Sudetenland; in 1947 there were population transfers between Hungary and Slovakia of Hungarians and Slovaks who lived in each other's countries. If Janics's approach were more detached, and if he satisfied himself with simply telling the story from the sources available to him, this would be an excellent study. It would show Beneš for what he was, namely, a short-sighted, self-serving politician with little understanding of the moral issues at hand.

Janics is incensed, and rightly so, that the Hungarians as a nation were found ideologically guilty and made to pay for it; he does not hesitate, on the other hand, to paint the Slovaks with the same brush. Again, this detracts from the story that is told. The Slovak politicians of the postwar era were a self-appointed group who obtained public sanction only because no one else was permitted to contest the elections of 1946. They had thrown in their lot with Beneš and, in their struggle to get their own plans for Slovakia accepted by him and other Czech politicians, they had to make compromises. They bear the responsibility in Slovakia for the anti-Hungarian campaign, not the Slovak people.

There are innumerable problems with this account, which space does not allow me to examine. It should be mentioned, however, that Janics uses very few government documents and quotes primarily newspaper reports. Another source of documentation that is not consulted is the files of the Foreign Office. They are filled with diplomatic correspondence and reports on the subject and should be studied. They would help to make some of the distinctions that are sadly lacking in this essay. The story that the author wants to tell should be told but with more detachment and a better understanding of events.

STANISLAV KIRSCHBAUM
Glendon College
York University

WILLIAM M. BATKAY. *Authoritarian Politics in a Transitional State: Istvan Bethlen and the Unified Party in Hungary, 1919–1926*. (East European Monographs, number 102.) Boulder, Colo.: East European Monographs; distributed by Columbia University Press, New York. 1982. Pp. 169. \$17.50.

The political and social fabric of the Hungarian state took over a thousand years to evolve, but long dependence on the Habsburgs and Austria prevented it from developing a sense of direction. After the realization of the age-old dream of independence at the end of World War I, the leftist-liberal Károlyi regime, the Communist Council Republic, a Social Democratic trade union government, and then Admiral Horthy's counterrevolutionary, traditionalist governmental system followed each other in short succession. The process of change and dissolution was augmented by grave territorial losses to the Romanians, Czechs, and Serbs. Under such circumstances many Hungarians became passively contemplative, while others demanded a policy of revenge against the representatives of the fallen regimes. Loyalist forces wanted to return the last crowned king, Charles IV, to power despite the opposition of the victorious Allies. The formation of national consensus on major issues became a hard task for politicians of any persuasion. After initial attempts by Friedrich, Huszár, Simonyi-Semadám and Count Teleki, Count István Bethlen took over as prime minister.

As the title indicates, William M. Batkay devotes considerable space to Bethlen's role in the formation of the Unified party, as well as to the political talents of the man whose life started in 1874 in a Transylvanian castle, and ended in Moscow's Lubianka prison in 1946. He rightly calls Bethlen a shrewd and cynical politician who "emasculated the Smallholders' [party], and built up an obedient and strong state machine to perpetuate his rule" (p. 45). We must add, however, that the Bethlen regime definitely was an improvement over the Bolshevik terror and the subsequent "White" counterterror, which were responsible for the execution of hundreds of people. One of its first tasks was the curbing of the extreme right. In the author's opinion, the reactionary aspects of Bethlen's policies appeared in the reduction of the land reform and secret balloting (it was allowed only in major cities) and the rigid fossilization of social and economic conditions. Batkay also accuses Bethlen of chauvinism (p. 11), which was hardly Bethlen's official policy. After the disintegration of the Habsburg monarchy, in each of the new "successor" states there prevailed a narrow form of nationalism; in Hungary, it was combined with a demand for territorial revision. In the first years of Bethlen's premiership, however, when reconstruction was the goal and Hungary was under foreign economic and military control, the activities of the government were not characterized by zealous and belligerent patriotism.

The book's major strength is the surveying of the organization and the functioning of the Unified

party, as the author calls it. (United party perhaps would be a better translation of *Egységes Párt*.)

Batkay could not avoid giving a historical summary, but it is distorted by omissions. One cannot describe the "origin and goals of the Hungarian counter-revolution" on three pages (from 9 to 11). The characterization of politicians is sometimes inaccurate. Friedrich is depicted as a conservative (p. 9); he was basically a turncoat. In 1918, one year before the formation of the counterrevolutionary government, Friedrich was an ardent leftist radical supporter of Count Károlyi and became assistant war minister in Károlyi's first cabinet. The Trianon Peace Treaty of 1920 is barely mentioned, although it determined Hungarian politics more than any other development for two decades. The franc-counterfeiting affair of 1925–26, which jolted the entire Hungarian political system by its international implications, and Prime Minister Bethlen's role in it, is also given only scant attention. The limitation of the admission of Jews to universities (the so-called *numerus clausus*) is incorrectly assigned to Bethlen (p. 147), since the law was promulgated under Count Teleki's premiership.

Some politicians are erroneously identified. Finance Minister Kállay (p. 75), who served in the Bethlen government from 1921 to 1924, is not identical with Miklós Kállay, the controversial World War II prime minister, as the index indicates; his name was correctly Tibor Kállay. The author's occasional dependence on outside opinions leads him to one-sided conclusions. For instance, the statement explaining that "interwar Hungarian writers and earlier Western historians were hampered by a lack of knowledge of modern political science" (p. 95), seems to be a superficial judgment.

Despite its shortcomings, Batkay's book, as a whole, treats the first half of the Bethlen era and the role of the Unified party with objectivity and critical understanding. He has produced a significant work of political science, which is especially important for those who do not have access to sources published in Hungarian. A second, revised edition should correct the errors and will also provide a more complete historical framework, which is necessary for the understanding of counterrevolutionary Hungary.

SANDOR SZILASSY
Glassboro State College

YISRAEL GUTMAN. *The Jews of Warsaw, 1939–1943: Ghetto, Underground, Revolt*. Translated by INA FRIEDMAN. Bloomington: Indiana University Press. 1982. Pp. xviii, 487. \$24.95.

The destruction of the magnificent synagogue of Warsaw by the Nazis on May 16, 1943, denoted the

end of Europe's largest Jewish community and one of the world's leading centers of Jewish learning and culture. While the demolition of the synagogue signified the Nazis' "victory" over Warsaw's Jewry, it also has come to symbolize the heroism and resistance of the remnant of one of the many forsaken Jewish communities of Nazi-dominated Europe.

This scrupulously objective and fully documented work chronicles the struggle of the Jews of Warsaw from the outbreak of the war on September 1, 1939, through the crushing of the revolt in May 1943. Yisrael Gutman identifies the purpose of this work as follows: "To examine the character and conduct of the Jewish community of Warsaw in face of the persecutive tactics of the Nazi occupation regime; to throw light on the means that were adopted to cope, both intellectually and psychologically, with the grave problems of the period; and to analyze the development of the armed resistance movement and the armed struggle of the Jews of Warsaw." He achieves these goals in an admirable and convincing fashion. Relying on a wealth of primary and secondary sources in a variety of languages, including of course German, Hebrew, Polish, and Yiddish, Gutman describes and dispassionately analyzes the motivations and conduct of the three collective actors in this great historical drama—the perpetrators and their accomplices, the victims, and the onlookers. His account begins with a succinct and highly informative introduction, offering an overview of the origins and development of the Jewish community of Warsaw. The fifteen chapters of the work are divided into three parts. Part 1, incorporating the first three chapters, is devoted to the ghetto, describing the first months of the Nazi occupation, the relations between the Jews, Germans, and Poles, and the character and scope of the Jewish institutions of Warsaw. It also provides a detailed account of the establishment of the ghetto, the organization, composition, and leadership of its various institutions, including the Jewish Council (*Judenrat*) and the ghetto police, as well as a description of the problems and horrors daily confronting the ghetto dwellers—30 percent of the city's population crammed into 2.4 percent of Warsaw's territory. The population of the Warsaw ghetto was 380,740 in March 1941 and reached its peak two months later when it increased to 445,000. By the time the deportations began in July 1942, the population was down to 335,514. As documented by Gutman, the fluctuations of the ghetto population were due primarily to the resettlement of the Jews from the neighboring communities and to the extremely high death rates within the ghetto.

The six chapters of part 2 describe the political underground in the Warsaw ghetto, including the various youth movements, the impact of the *Einsatzgruppen* massacres in the German-occupied

parts of the Soviet Union, and the mass deportation of the Jews between July 22 and September 12, 1942. During these seven weeks, close to two hundred sixty-five thousand Jews were deported to Treblinka, where they were gassed and cremated shortly after arrival. Separate chapters deal with the complex questions relating to the establishment by representatives of the major Zionist youth movements of the Jewish Fighting Organization (*Żydowska Organizacja Bojowa*)—a merger of political and ideological factions that transcended traditional conflicting philosophies, the response of the Poles during and in the aftermath of the massive deportations, and the structure and life of the shrunken ghetto during the remaining months of 1942.

Part 3, composed of six chapters, is devoted to the revolt. It describes in detail the preparations for the uprising, which were intensified after the Germans launched their second *Aktion* against the Jews of Warsaw (January 18–22, 1943) that claimed an additional 6,500 lives. The author also provides a vivid day-by-day account of the battle as viewed by both sides, quoting poignantly from the reports and diaries of many of the *dramatis personae*, including Mordecai Anielewicz, the leader of the uprising, Chaim Frimмер, Adam Halperin, and Marek Edelman, as well as General Jürgen Stroop, the commander of the German forces. The battle was obviously fought by an uneven alignment of forces, but the self-sacrificing heroes, unaided by the Poles and forsaken by the rest of the world, fighting with a few obsolete and homemade weapons, managed to withstand the relentless onslaught of the German war machine for almost a month. With the passage of time, it becomes increasingly clear that the Warsaw Ghetto uprising of April 19, 1943, was one of the most remarkable events of World War II, reflecting the triumph of the spirit of freedom over the forces of barbarism. It provided one of the most shining pages in Jewish history and in the history of man's inhumanity to man. It is gratifying to know that this admirable book will be on the shelves of libraries as a source of documentation.

RANDOLPH L. BRAHAM
City College
City University of New York

V. L. IANIN. *Novgorodskaja feodal'naja votchina: Istoriko-genealogičeskoe issledovanie* [The Novgorodian Feudal Votchina: A Historical-Genealogical Investigation]. Moscow: Nauka. 1981. Pp. 295. 1 r. 20 k.

V. L. Ianin is now clearly the pre-eminent living historian of medieval Russia. Known especially for his work on Novgorod, he has mastered several disciplines auxiliary to the historian's preoccupation

with written sources. He is numismatist, archaeologist, and historical geographer, to name only a few of his achievements. All this training has prepared him to employ what he calls here "a new aspect of the study of sources."

Ianin's latest contribution is a genuine masterpiece that includes numerous subjects of interest. Ianin adopts the view that since landed estates were divided at inheritance, the existing cadastral records contain "the final result of the prolonged process of family divisions, a [process] equivalent to the physical development of the families themselves" (p. 5). Consequently, with the aid of the fifteenth-century Novgorod cadasters and other written sources, including the excavated birchbark charters, Ianin persuasively "reconstitutes" several Novgorodian lineages. Although almost certainly incomplete, the genealogies are remarkable simply because they so persuasively bring to the historical narrative real, particular families of medieval Russia.

It is easy for the reader to become so interested in Ianin's excavation of these families that he forgets that the book is directed toward discussing Novgorod's landed estates. Two themes have long dominated the historiography: the origins of "feudal" estates and the origins of the small freeholdings (*svoezemtsy*). Ianin has interesting and generally convincing things to say on both issues. Before the twelfth century, he argues, rights to landed property belonged exclusively to the princes, who exercised only rights of domain. By the middle of the fourteenth century, large landed estates had become a predominant feature of Novgorodian landholding, and private, nonprincely landowners (Ianin calls them feudal lords) controlled much of the real estate in the Novgorod lands. Division at change of generations, however, meant a gradual decrease in the size of privately held plots for most families. Small freeholders were the unfortunate heirs of estates that had been subdivided many times over the course of several centuries. At the opposite end stood those few landholders, generally church institutions, who took advantage of the poverty of their peers to enlarge their own estates.

Also interesting, and undoubtedly destined to become controversial, is Ianin's discussion of landholding in the period before the princes established large estates. Ianin speculates that before the eleventh century, when the princes seem to have come into possession of these properties, the city assembly (*veche*) held the land as indivisible state property. Gradually the city boroughs (*kontsy*) acquired rights over this land in tandem with the growth of municipal administration. Indeed, even in the fourteenth and early fifteenth centuries members of Novgorod's elite held the right to collect incomes from some of these estates without actually holding title to the land. Or, as Ianin puts it, "state landholding

becomes a synonym of corporate boiar possession of these lands" (p. 279).

There is much more here that demands the attention of all historians of Russia. Both in interpretation and in method the book marks a turning point; it will endure as an example of how to do history and as a literate and thorough reinterpretation of some controversial themes.

DANIEL H. KAISER
Grinnell College

ANDREAS KAPPELER. *Russlands erste Nationalitäten: Das Zarenreich und die Völker der Mittleren Wolga vom 16. bis 19. Jahrhundert*. (Beiträge zur Geschichte Osteuropas, number 14.) Cologne: Böhlau. 1982. Pp. vii, 571. DM 158.

Of all the minorities of the Russian empire, the Kazan (Volga) Tatars have attracted the most able historians. The studies by Jaroslaw Pelenski, Alexandre Bennigsen, Chantal Lemerrier-Quelquejay, and Helene d'Encausse on the pre-Petrine and postrevolutionary periods remain the most successful treatments of the empire's non-Russian peoples. Now Andreas Kappeler has filled the gap between the annexation and the modern period with a masterful study.

Kappeler traces the socioeconomic and demographic development of the Volga minorities from the sixteenth century to the era of the great reforms in the 1860s. During this period the government made every effort to restructure the entire region along Russian lines. It assaulted the local, largely Muslim, culture through the Orthodox Church with forced conversion and missionary activity, and used both force and fiscal measures to encourage cultural assimilation. With a well-constructed land, colonization, and tax policy the Russians tried to undermine the economic foundations of these people's traditional society. And finally, by removing all distinctions still retained by the local nobility in the eighteenth century, the government struck what should have been a fatal blow to Tatar society.

The government was motivated at first by a deep hostility toward Islam. In the eighteenth century, the desire to establish uniform social and economic institutions throughout the empire overrode religious considerations. The administrations of Alexander I and Nicholas I combined both cultural and social goals to press for assimilation of these peoples into the Russian mainstream. But by 1860, after three hundred years of such efforts by a supposedly more advanced and developed civilization to eliminate the more backward culture, the Russians had apparently failed. For the late decades of the nineteenth century witnessed the powerful awakening of the Volga minorities, led by the Kazan Tatars, which

would result in the early twentieth century in a nationalism based on Tatar and Islamic culture that survives even today.

Kappeler provides a masterful examination of the Russian efforts over this long period. The reader's difficulty in understanding their failure to achieve their objectives is, in part, the result of the source materials used by the author, and the lack of focus on the native cultural resources of the region's peoples. Kappeler has depended entirely on published materials, though he has utilized virtually everything that has appeared in Russian, both of a primary and secondary nature. The fact that non-Soviet historians are denied access to the originals when the subject concerns any form of minority or nationality questions, even for the early period, forces us all to depend on the accuracy, honesty, disinterestedness, and skill of the documents' compilers. For the study of any other area of history, this situation would be unacceptable.

But just as importantly, Kappeler has not made use of the rich materials written, and in many cases published, by the Volga peoples in their own languages. For it is here that the historian may find the necessary clues to the Russian failure. The strength and vitality of Tatar culture is not easily transmitted in Russian translation. Halen's *Handbook of Oriental Collections in Finland* identifies more than a thousand published items in Kazan Tatar from the prerevolutionary period, a substantial part of which relates to the history of the region, socioeconomic and cultural development of the Volga peoples prior to 1860, and the history of Russian-Tatar relations in Kappeler's period.

This criticism aside, all historians of the Russian empire will have to read Kappeler's book. The other non-Russian nationalities deserve scholarly treatment as competent as the Volga Tatars have received.

ALAN FISHER
Michigan State University

E. I. DRUZHININA. *Iuzhnaia Ukraina v period krizisa feodalizma, 1825–1860 gg.* [The Southern Ukraine in the Period of the Crisis of Feudalism, 1825–1860]. Moscow: Nauka. 1981. Pp. 214. 2 r. 60 k.

E. I. Druzhinina, foremost Soviet historian of the late eighteenth and early nineteenth centuries specializing in Russia's expansion toward the Black Sea, established her reputation with three previously published monographs—*The Kuchuk Kainarji Treaty* (1955), *The Northern Black Sea in 1775–1800* (1959), and *The South Ukraine in 1800–1825* (1970). This study depicts events discussed in the latter book and ends with the year 1860, predating the abolition of serfdom and the end of "feudalism" in Russia as it

has been customarily defined in Soviet historiography. The author's concentration is focused on the socioeconomic development of the area formerly known as "New Russia," comprising the Kherson, Ekaterinoslav, and Tavria (Crimean) *gubernii* that now, with only minor alterations, make up the southern Ukraine.

This territory was conquered by Russia during the Russo-Turkish war of 1768–74, and was contested for decades by the Zaporozhian Cossacks and Crimean Tatars. By 1774 the entire area fell under Russian domination, thereby lending access to the Black Sea as well as permitting economic integration and settlement of the most fertile regions of the Ukraine. Its contribution to overall Russian economic growth and a detailed discussion of the changes and progress made in the area are the subject of Druzhinina's penetrating study, based on original sources and documents from archives and libraries.

To this end Druzhinina developed her research within four main topics: population, agriculture, industry, and domestic and foreign trade. Having access to original statistical data pertaining to all chapters, she included in the text twenty-one tables that are analyzed and, in fact, the whole discussion revolves around statistical information in most of the cases for the period. Exactly this aspect of the study will be appreciated most by Western scholars who are restricted in their research of original sources. In each case the sources are identified, making additional research as well as verification possible.

The chapter on population and settlement reveals that Ukrainians were most prevalent in number, followed by Russians, and then by some two dozen other ethnic groups, including German colonists, Serbian and Croatian settlers, Greeks, and Jews. By 1858, the southern Ukraine was the most heterogeneous part of the Russian empire with a population of 1,331,509 men (women were not included). The largest social class was the peasantry (1,596,000 free-settlers, serfs, and colonists; state serfs and peasants numbered 501,710).

The chapter on agriculture, for the area of 244,000 square kilometers, offers data on various crops and the potato harvest for the years 1842–60, broken down by private and state-owned lands. It also contains information on the introduction of new cultivations, such as grapes and tobacco. The area underwent significant changes during the period discussed, moving from an originally livestock-oriented economy to a predominantly crop-cultivating area that accounted for 32.2 percent of Russia's agricultural exports by 1860. This accomplishment alone signifies the importance of the newly incorporated territory along the Black Sea for Russia's economy and indicates how the Ukraine became the

breadbasket of Europe before the Bolshevik revolution.

The chapter on industry, a less successful undertaking, ties industrialization to wars with the Ottoman empire. For reasons of national interest industry emerged primarily as large state-owned factories, which declined rapidly after the Crimean War of 1853–56.

Considering all aspects and facets of the area's development and character, Druzhinina's characterization of the period as a "crisis of feudalism" cannot be sustained. Obviously, the development of the southern Ukraine during this period must be seen as an agricultural and commercial revolution—certainly not feudalism, though remnants of the latter were still present in Russia proper.

The author's stress on (and warm approval of) Russian patriotism is worthy of mention. Name and geographical indexes are helpful, as are citations to Soviet secondary literature in the footnotes. The economic historian of Russia will find this study useful, especially in updating his sources and statistical data.

STEPHAN M. HORAK
Eastern Illinois University

E. N. BABIKOVA. *Dvoevlastie v Sibiri* [Dual Power in Siberia]. Tomsk: Tomsk University Press. 1980. Pp. 157. 1 r. 50 k.

Dvoevlastie, a Russian term usually translated as "dual power," refers to the simultaneous existence of Provisional Government and Petrograd Soviet after the collapse of the monarchy in March 1917. It is a convenient concept for teachers and scholars because it corresponds with apparent reality; that is, Duma liberals formed and dominated the early Provisional Government while socialists formed workers' councils, or soviets. The latter distrusted the bourgeoisie, but agreed to support the government as long as it pursued acceptable policies. While government and soviet leaders simultaneously competed and cooperated in the early stages of 1917, revolutionary forces were at work under the surface. Lenin, more prescient, decisive, and ruthless than other leaders, harnessed these forces and seized power in November.

Marxists have had more than a casual interest in *dvoevlastie*. Mensheviks viewed it as a transitional period calling for compromise. The March Revolution inaugurated a bourgeois phase during which the proletariat, unready to wield power, could protect its interests through the soviets. Bolsheviks, however, viewed the conciliationist Mensheviks and Socialist Revolutionaries as renegades from the proletarian cause and thus did not regard this as true *dvoevlastie*. Still, they credit the soviets with prevent-

ing the bourgeoisie from ruling alone. Bolsheviks view the *dvoevlastie* as ending in July when the unrest of the "July Days" caused the Provisional Government to attempt to rule through a military dictatorship. And because the Mensheviks and their fellow renegades supported the government anyway, the cabinet reshuffle in late July, which gave them a preponderance in the government, did not alter this Bolshevik view.

Soviet historian E. N. Babikova opens her brief monograph with the terse statement, "The main feature of the second Russian bourgeois democratic revolution was *dvoevlastie*," a term, she says, that first appeared in the contemporary bourgeois press. Lenin was concerned with this phenomenon. Yet, according to Babikova, Soviet historians did not give the March Revolution and *dvoevlastie* serious attention until 1967, their fiftieth anniversary. Since then numerous Soviet scholars have produced serious studies of this question. Although ideological proclivities, writing style, and mechanical deficiencies (for example, absence of indexes and bibliographies) continue to render Soviet works less satisfactory, Western scholars welcome the added data and increased sophistication of many of these works, including this book.

The author has attempted to clarify the nature of *dvoevlastie* in Siberia by applying a Leninist analysis to the relationship of political groups to each other and to the complex events. The predictable conclusion is that one cannot speak of a bona fide *dvoevlastie* in Siberia in the March to July period. The whole political process of 1917 lagged, not only because of Siberia's distance from the center but also because of its lower level of economic development. In agrarian Siberia, peasants displayed their petty bourgeois mentality and appeared more interested in seizing land than power. In the cities and towns bourgeois elements were forming committees to replace tsarist authorities. Socialist leaders, inclined toward conciliationism, were attempting to organize the workers who were weak in numbers and class consciousness. Siberian Bolsheviks were confused and remained in united Social Democratic organizations longer than in western Russia. Soviets were organized, but remained embryonic until the Bolsheviks gained ascendancy and created true *dvoevlastie*. This occurred later in Siberia, but by this time the Bolsheviks were using the soviets to achieve full power, or *edinovlastie*—a condition actually more comparable to the autocracy they had so vigorously opposed.

CANFIELD F. SMITH
University of Alabama,
Birmingham

L. V. IVANOVA. *Formirovanie sovetskoi nauchnoi intelligentsii, 1917–1927 gg.* [The Formation of the

Soviet Scholarly Intelligentsia, 1917–27]. Moscow: Nauka. 1980. Pp. 390. 2 r. 40 k.

In recent years the formation of the Soviet scholarly intelligentsia has become a topic of considerable interest to Soviet historians. Serious studies on the subject have been published by E. N. Gorodetskii, A. V. Kol'tsov, S. A. Fediukin, M. S. Bastrakova, and V. A. Ul'ianovskaia. In the United States, too, the subject has attracted its historians: David Joravsky, Sheila Fitzpatrick, Kendall Bailes, and Loren Graham. Now L. V. Ivanova, a Soviet historian who has previously published on the topic, has produced yet another ambitious study, one that heavily utilizes Soviet archives.

Ivanova organized her book around four major themes: the impact of the October Revolution on the prerevolutionary Russian academic community; the role in transforming the Soviet intelligentsia of the small number of scholars who were also members of the Communist party; the social and political life of members of the Soviet intelligentsia in the first ten years of the Soviet regime; and the changes in the composition (nationality, sex, social origin) of the Soviet intelligentsia in the same period.

The result is a book that contains a great deal of interesting information, yet one that is slanted in such a way that it must be used with caution. Some of the statistical, biographical, and organizational data taken from the archives is extremely informative. Yet one's enthusiasm for the information diminishes when one notices that such an important figure as Nikolai Bukharin has been carefully excised from the entire book, even though he was a leader in many of the activities being described. Ivanova has provided a list (pp. 97–103) of the holders of important intellectual, academic, political, and publishing posts throughout the period 1917–27, from which she has eliminated all people who were condemned in the Stalin era and not subsequently rehabilitated. Stalin's name appears, as it does elsewhere in the book, but Bukharin never makes it—not even under the listing of the staff of the main party newspaper, *Pravda*, despite the fact that he was editor-in-chief during the period. Other examples could easily be given. And the problem is not simply one of omission of people's names; much of the book is not history but a justification of the government and party policies.

Despite these serious flaws, the book makes a contribution to our knowledge of the subject. Only two examples can be given in a short review: (1) the discussion of the formation of unions of scholars and (2) the statistical data on the composition of the Soviet intelligentsia. The book contains one of the fullest Soviet discussions of the ways in which the first unions of Soviet academicians, the Union of Academic Institutions and Universities and the

Union of Professors and Teachers, were gradually destroyed by the Soviet authorities and replaced by the Section of Scientific Workers of the regular trade unions. The fate of trade unions for Soviet factory workers is well known in the existing Western literature, but relatively little is yet known about the academic unions.

On the second topic, statistical data on the Soviet intelligentsia, the book contains valuable material. In 1928, over a decade after the revolution, a special statistical survey of the Soviet intelligentsia showed that specialists with prerevolutionary educations and experience were still most numerous in military fields (64.2 percent) and in literature (58.9 percent). If one divided the fifteen thousand "scientific workers" into three categories of academic reputation of "outstanding," "established," and "beginning," only 9 of the 487 in the first group were members of the Communist party, compared to 506 of 9,212 in the third group.

The picture that emerges from the book is of a Soviet academic intelligentsia in 1927–28 in which the top ranks had changed very little since the revolution. The lower ranks, however, were gradually filling with young, militant party members, setting the stage for the "great break" that came at the end of the period, outside of Ivanova's study.

LOREN R. GRAHAM
Massachusetts Institute of Technology

I. I. MINTS and A. P. NENAROKOV, editors. *Zhenshchiny—Revolutionary i uchenye* [Women Revolutionaries and Scholars]. Moscow: Nauka. 1982. Pp. 188. 70 k.

This memoir collection focuses on three Bolshevik women—A. M. Pankratova, S. A. Ianovskaia, E. K. Sokolovskaia—who worked in Odessa during the Civil War and later became, the editors assert, teachers and scholars. Some qualification is immediately required in the case of Sokolovskaia, for she was never a scholar but rather was a career Bolshevik. The editors, I. I. Mints and A. P. Nenarokov, attempt to justify her inclusion on the grounds that she worked at Mosfilm in the 1930s, but she was there as a political administrator.

Sokolovskaia is also distinguished from the others by her fate. Her husband, Ia. A. Iakovlev, head of the Central Committee Agricultural Department, was executed in 1938. Later that same year Sokolovskaia died. Although this collection neither gives the cause of death nor mentions Iakovlev's, the reader may assume that had she died naturally, the editors would have said so.

Pankratova and Ianovskaia survived Stalin's purges. Of the two, Pankratova has the greater claim to scholarly accomplishment. A schoolteacher

turned Bolshevik during the revolution, she became a student of the Marxist historian M. N. Pokrovskii in the 1920s, and pioneered the study of the history of the proletariat. When such history fell from favor in the 1930s, Pankratova was hounded and briefly exiled. After Stalin's death she worked successfully for the rehabilitation of Pokrovskii's reputation. As a scholar and a teacher she played an important role in training a new generation of Soviet historians.

Ianovskaia also suffered attacks in the 1930s, occasioned in her case by her advocacy for the field of mathematical logic. Like Pankratova she came to Bolshevism during the revolution and like Pankratova she went on to the university in the 1920s. More a teacher than a scholar, Ianovskaia promoted the study of logic (condemned in the 1930s as un-Marxist) and assisted the development of cybernetics.

These memoirs illuminate the women's lives only if one is alert to nuance. Whether anyone other than a specialist on Bolshevik women would profit from plowing through the fulsome praise of these articles looking for kernels of truth is debatable. What we have here is two hundred pages of carefully but selectively footnoted hagiography. The memoirists—colleagues, fellow revolutionaries, old friends—portray the three as Bolshevik women are always portrayed: they were brave, selfless, dedicated, modest, principled, and caring. Of course there is some truth to this characterization, and women who fought in the Civil War and then achieved prominence in Moscow did have extraordinary qualities. But the complete story of their heroism is not told here. One has to read carefully to realize that Pankratova and Ianovskaia were fighting for their professional lives much of the time, or that Sokolovskaia died because of the purges. The women's private lives are kept so private that Sokolovskaia's and Ianovskaia's husbands are mentioned only in passing, Pankratova's not at all. Any information about Russian women is welcome, but one wishes that a generation after Stalin's death his malignant influence on Soviet historiography would cease. Then perhaps Bolshevik women could be removed from the cotton batting of inhuman virtue into which they continue to be so lovingly and fearfully packed.

BARBARA EVANS CLEMENTS
University of Akron

NEAR EAST

MALCOLM CAMERON LYONS and D. E. P. JACKSON. *Saladin: The Politics of the Holy War*. (University of Cambridge Oriental Publications, number 30.) New York: Cambridge University Press. 1982. Pp. viii, 456.

The authors of this most recent excursion into the life of a medieval luminary have produced a meticulously researched analysis of the military campaigns and supportive statecraft that together constituted the career of Salāh al-Dīn Yūsuf b. Ayyūb—known to the West as Saladin, a leading figure of the Crusader era. The complex web of diplomacy, intrigue, and expeditions that confronts the reader is set forth in the larger context of social forces and political attitudes that shaped the objectives and vision of state held by Saladin and his contemporaries during the twelfth century. M. C. Lyons and D. E. P. Jackson have justified their prodigious effort on the basis of sources hitherto untapped in any systematic way. In addition to chronicles of such writers as al-Isfahānī, Ibn al-Athīr, William of Tyre, Abū-Shāma, and Ibn Shaddād, they have scrutinized collections of letters—most compiled by one of Saladin's closest confidants, his personal secretary and vizier al-Qādī al-Fādīl—to seek out Saladin's own perspective on his actions, as well as those of his advisers. The insights these letters provide to our understanding of Saladin's policies comprise the study's original contribution.

But throughout their narrative, Lyons and Jackson have also posed an old question in a new frame of reference. Was pursuit of the holy war against Christian infidels the ultimate goal of Saladin's career, or was it an expedient he exploited to manipulate individuals and groups from a highly fragmented and unstable Muslim community who otherwise might have withheld their allegiance? The authors opt for Saladin's sincerity of purpose, as they state in the conclusion (p. 370), but they show quite convincingly how such an objective could be realized only with an acute sense of the patronage necessary to bind followers, and to extract from them their best efforts. Saladin always valued men above money, and the essence of his statesmanship is most visible in his penchant for rewarding followers substantively with gold or land. Yet Saladin's generosity in matters of money led him to the brink of insolvency on several occasions, setting a precedent for the endemic fiscal exploitation that marred the policies of his militarist successors in Egypt and Syria during subsequent periods.

The study begins with Saladin's seizure of the vizierate in Egypt and his displacement of the Fātimid Caliphate, all the while maintaining the fiction of his superior's suzerainty, Nūr al-Dīn of Aleppo. Lyons and Jackson stress the role of felicitous circumstance in these formative years, and yet Saladin's growing political acumen is amply demonstrated. Less clear is Saladin's own personality during his early life, due to the sources' failure to provide a vivid portrait of him, but the authors speculate perceptively on his education (pp. 3–5). The study then proceeds through Saladin's first

expeditions into Syria, ending with the fortuitous death of Nūr al-Dīn; his consolidation of authority over the contending principalities of Syria, culminating in the submission of Aleppo; his series of military campaigns against the Europeans (Franks), aimed at defeating the Latin Kingdom of Jerusalem and marked by his greatest victory—the Battle of Hattīn; and his ultimate failure to drive out the Frankish presence, symbolized by his peace agreement with Richard I of England.

Throughout this detailed portrayal, the authors never lose sight of their underlying argument. The propaganda of holy war enabled Saladin to rally otherwise fractious Muslim elements behind a cause with which they could identify. But although Saladin exploited this cause successfully in the short run, he proved to be incapable of creating a new kind of political order in its aftermath. He founded a viable dynasty but no cohesive state. Indeed, in light of reactions by such figures as the Abbāsīd caliph al-Nāsir, the titular head of orthodox Islam who regarded Saladin as a potential usurper, such an achievement exceeded his capacity. Moreover, Lyons and Jackson do not attribute strategic brilliance or profound intellectual depth to this man. Rather, they interpret him as one who “held instinctively to the middle ground” (p. 374). Capable of inspiring deep loyalty among his associates, Saladin did not transcend the ideological limits of his age.

Barring discovery of new evidence, Lyons and Jackson have written what is probably a definitive statement on Saladin's career. The price a reader will pay for absorbing it involves his wading through a dense narrative periodically enlivened by the authors' reflection on events. These analytical and anecdotal pauses, notably Saladin's administration of Egypt (pp. 54–57); the antics of his aides in Damascus, luridly described by Ibn Unain (p. 119); diplomas regulating crafts and professions (pp. 203–04); and Saladin's failure to create a pan-Islamic empire (pp. 239–40), grant relief from the stream of minutiae by reminding the reader of their broader significance. Also, the authors have not elected to engage their scholarly predecessors in open debate. One must refer to the notes to weigh their explicit disagreements with H. A. R. Gibb and A. S. Ehrenkreutz, none of which are major. The challenge of this book to earlier views is thus implicit, and the responsibility for finding it lies with the reader seeking to make sense of the intricate tapestry of Crusader historiography.

To conclude, the authors have maintained a high level of accuracy while transcribing the myriad Arabic names and terms in their text. Furthermore, as an advocate of cartographic sources in my own work, I welcomed the series of detailed maps at the end of the book. But I should note that the scale to the Plan of Cairo (p. 375) cannot possibly be correct.

Meters/yards rather than kilometers/miles would more closely approximate the actual distances.

CARL F. PETRY
Northwestern University

GABRIEL BAER. *Fellah and Townsman in the Middle East: Studies in Social History*. London: Frank Cass. 1982. Pp. ix, 338. \$36.00.

This volume groups some of Gabriel Baer's recent articles into four sections. Part 1 (three chapters) explores relationships, including qualitative differences, between town and village. Part 2 (two chapters) traces the office of village shaykh (then mukhtar) in Palestine through the Ottoman period and British mandate into contemporary Israel and Jordan. Part 3 (four chapters) treats the nature and function of Turkish guilds. Part 4 (two chapters) investigates popular revolt in urban and rural settings. Within chapters, subheadings are typically thematic—for example, on revolts: causes, participants, leaders, aims, and achievements; comparisons or contrasts are regularly made within the region and with Europe. The result, methodologically, is history as "sociology of the past" rather than as narrative.

Baer feels that "overgeneralization," whether stemming from interpretive grand designs or from tendencies to attribute to other regions and periods the conclusions of one narrow study, has obscured urban-rural issues in Middle Eastern history. He consequently avoids overarching theories and formulates only such modest conclusions, many frankly revisionist, as his evidence supports—for example, that, far from extolling fellah life, a seventeenth-century Egyptian text actually satirizes it; that modernization did not narrow but, in the nineteenth century, widened urban-rural differences; that Gibb and Bowen, among others, confused *futuwwa* organizations with professional guilds; that the ulema functioned not as leaders of urban revolts but instead, under duress, as communicators of popular demands.

Baer carefully links his own conclusions to specific times and places: the mukhtar did not effectively replace the village shaykh in Palestine during the Ottoman period; guilds arose in Ottoman towns under official patronage (Bursa may be an exception) and functioned as administrative links between the government and the urban population; Cairo's rioters during 1798–1805 consisted of the urban-suburban poor (supplemented by Azhar students) led by figures who had established patron-client relationships with particular groups; the 1858–61 revolt of Maronite peasants in Lebanon exceptionally aimed at changing the socioeconomic order and not just at redressing grievances. Interpretive cau-

tion is appropriate for another reason; although their sources are many and varied, Baer's studies (excepting the two on the mukhtar and, indirectly through Nuri's published collection of documents, those on the Turkish guilds) do not issue primarily from direct contact with the Middle East's still largely neglected treasure of archival materials. An overarching theory of social history based on anything less would be premature.

The volume shares three problems of its genre (anthologies of reprinted articles). First, although one-third of the material is reputedly new, the new writing (excepting the piece on fellah revolt, one of the longest and best) usually represents not fresh research but bridge or summary additions to the ten republications. Another problem is unity: parts 1 and 4 harmonize nicely, but parts 2 and 3 blend less well with the rest of the volume, although the categorization schema and the index are unifying features. A final problem is redundancy: separate articles on various aspects of a common theme contain some repetitions when consolidated; particularly in parts 1 and 2 the reader encounters identical information and conclusions—sometimes identical passages—in each chapter. These problems of form, however, are more than offset by the quality of the substance and by the convenience of having the related short studies of one of the Middle East's best social historians available in a single volume.

ARNOLD H. GREEN
American University
Cairo, Egypt

MANGOL BAYAT. *Mysticism and Dissent: Socioreligious Thought in Qajar Iran*. Syracuse: Syracuse University Press. 1982. Pp. xvii, 228. \$25.00.

Many books on the Iranian revolution of 1978–79 and its causes have recently been published, as well as numerous articles on postrevolutionary Iran. Despite this flurry of publishing, however, readers who wish to understand the causes of the revolution may still find themselves lost in the woods. The need for a guide to lead them out of the thickets of polemic and misinformation has now been amply met by Mangol Bayat.

Bayat's book, *Mysticism and Dissent*, explores the causes of the revolution by a careful study of the past; the work is mainly a study of the Imamate in Shi'a Islam and the attempts to dissent from it in the nineteenth century. Bayat provides a comprehensive account of the development of Shi'ism and examines the later dissent within it. He discusses the cyclical progressions of dissent from the time of the first "constitutional crisis" in the Islamic community, when the Dar al-Islam split into the Sunni and Shi'a

branches, until the recent return to an exaggerated form of the early tradition—that is, the political movement aimed at achieving the goals of the respective leaders.

By tracing the dissident elements of Shi'ism through traditional dissent, and particularly through the radicalization of early *Shaikhism* and its socialization in Kirmani *Shaikhism*, then through the politicalization of *Babism* to its ultimate secularization, Bayat lays the groundwork for the triumph of secularization under the Pahlavis. The ultimate complete reversion to the dominant orthodox Shi'ism is then presaged by the fall of the shah and his ultramodernization, enabling the previously diminished but formally cohesive ulama to resume a position of power even stronger than before the nineteenth-century dissent began.

Despite its esoteric title, Bayat's book does not pretend to analyze the role of the Shi'a religion in the recent events of Iran. It does, however, provide a perfect backdrop for an understanding of the socioreligious chemistry of the revolution. In addition, a careful reading of the book gives one an understanding of the modus operandi of the Khomeini regime. The historic approach to the subject presents a consistent framework leading up to recent events.

Bayat's main contribution consists of disproving the belief of many scholars that the main thrust of dissent in the nineteenth century was political. Many believe that because the ulama considered the secular rule of the monarchs illegal (since they were governing during the ongoing occultation), most dissent arose from political tensions. The author shows, however, that the situation was in fact more complex. Tension as well as cooperation existed between the Shi'a hierarchy and some of the Qajar monarchs, but most of the dissent occurred within the religious structure itself. "The quest for a just social order and the aspiration for a world free from oppression transformed the political opposition into a sectarian religious movement, with serious doctrinal implications concerning the nature and function of the rightful leader referred to as the Imam."

Thoroughly researched, this complicated subject is written about so clearly that it easily avoids the pitfalls produced by the "bewildering labyrinth of religious politics in 19th century Iran, and the widespread practice of *taqiyya* [concealing one's true view from the public] and the art of esoteric writing" (p. 79). One receives the impression that the book was written by someone in the manner of the traditional German scholar who in the "Einleitung" describes in detail the goals that will be reached and the methods that will be applied. Mangol Bayat is faithful to this outline.

Although this is a specialist's book, it is still a must for all those who are interested in the current events

of Iran, and how they derived from previous events. The only drawbacks to this superbly written and researched work is some bias on the part of the author, who appears to give the *Bab* and his followers a somewhat less sympathetic treatment than that given to other dissidents.

PETER CHELKOWSKI
New York University

HAMID ENAYAT. *Modern Islamic Political Thought*. (Modern Middle East Series.) Austin: University of Texas Press. 1982. Pp. xii, 225. Cloth \$20.00, paper \$8.95.

The Islamic revolution in Iran has generated fresh interest in Islamic political thought. This book by Hamid Enayat, former professor of political science at Tehran University and lecturer until his recent death at Oxford, casts new light on traditional concepts and notions of political behavior among Shi'a Muslims.

With incisiveness and mature judgment the author discusses the roots of Shi'a-Sunni discordant political thinking, the role of the caliphate historically, views on reforming the Islamic state, Islamic and modernist notions of democracy, and the Shi'i conception of constitutionalism in the context of inherited behavior.

While giving Sunni political analysts their proper due, Enayat attributes the lack of commensurate political expression among Shi'i to pessimism concerning the attainability of the ideal polity before the millennium. This explains Shi'a toleration of "illegitimate authority" and the lack of political commitment. Shi'a notions of man's fallibility is a contributing factor. But Shi'a emotionalism, on the other hand is seen as a source of vitality when contrasted with "Sunni formalism." Admittedly such emotionalism historically was allied to political despotism in Iran, nevertheless preference favors tolerating "oppressedness" over defying established authority.

Unorthodox political behavior is ascribed to Muslims not treating the subject of politics independent of other disciplines, thus accounting for persistent factionalism. Mu'tazilis are lauded for injecting rational thinking into political thought. The Brethren of Purity are praised for introducing the idea of indicting society's wrongs and calling for doctrinal re-education. Sunni orthodoxy is criticized for suppressing such promising trends for the realization of a truly functional Islamic democracy today.

Amelioration is seen in the Sunni-Shi'a ending of the sanctity of status quo thought, the rejection of the corrupting realism of medieval writers, the revival of historical criticism, and the salvage of the democratic and socialistic elements of the Islamic past. To achieve this no caliphate is necessary. But in

mustering the facts, Enayat's arguments become weak. The same inconclusiveness characterizes his analysis of the Islamic state. Indeed, it adds little to standard works on the subject, particularly as concerns the roles of Rashid Riḍa and the Muslim Brotherhood. He does argue convincingly for the separation of "Islamic order" and "Islamic state" as interpretive ideas.

Enayat argues eloquently for the assertion of those reformist trends among Sunni and Shi'i that would discard considerations elevating race over community by recourse to eras antedating Islam among Persians and Arabs. He musters convincing data in the service of democracy and socialism latent to Islam, which would give the individual his proper due without compromising God's own unique existence and individuality—as feared traditionally by the religiously active.

Twentieth-century experiments in liberal democracy were bound to fail because of what Enayat considers a lack of conceptual coherence and an absence of specific social and economic formations, that is, a conscious, articulate middle class. Socialism is one modern ideological formulation that is seen as compatible with Islam's temper. Criticism is leveled against modern socialist thinkers among both Sunni and Shi'i for not translating radical indigenous Islamic ideas into a language understood by the masses.

While Enayat exhibits flashes of brilliant analysis, the last chapter raises questions concerning the advisability of resorting to apologetics in defense of traditional Shi'a institutions and their motives for political behavior. But this observation notwithstanding, the author must be commended for an exciting and challenging treatment of an important and timely subject of study. This reviewer does regret, however, the abruptness with which Enayat ends his writing. A summary conclusion would have aided the reader unfamiliar with the subject in overcoming what might appear to him as diffused and confusing in the understanding of trends in Islamic political thought.

CAESAR E. FARAH
*University of Minnesota,
Twin Cities*

ALAN R. TAYLOR. *The Arab Balance of Power*. (Contemporary Issues in the Middle East.) Syracuse: Syracuse University Press. 1982. Pp. xii, 165. Cloth \$22.00, paper \$11.95.

The quest for unity has absorbed the Arabs and their governments for most of this century. Along with the loss of Palestine, the failure to achieve, let alone to approximate, unity has been the Arab tragedy of the modern era. Despite a shared lan-

guage, culture, and history, and the push of such integrative forces as the communications revolution, pan-Arab ideology, joint Arab economic ventures, and the question of Palestine, the Arabs have been unable to resist political fragmentation. The persistence of ethnoreligious conflicts and primordial loyalties, the disparity between the human and material resources of different Arab states, and the unevenness of their development continue to act as obstacles to the achievement of Arab unity. But so does the consolidation of the individual Arab state, which refuses to subordinate its interests to those of the larger Arab system.

It is to the workings of the Arab state system that Alan R. Taylor of the American University devotes his most recent study. He traces the origins of the system and takes the uninitiated through the maze of shifting Arab alignments from the formation of the Arab League in 1945. He correctly places the climax of Arab unity efforts at the time of the union between Egypt and Syria in 1958. Its collapse in 1961, followed by the humiliating defeat of the Arabs by Israel in 1967, turned the dream of unity into a nightmare from which Arab regimes were only too glad to be awakened. By the late 1960s the Arab state system had suffered a seemingly irreparable breakdown. To rehabilitate it, Arab leaders adopted what Taylor calls the "new pragmatism," which he associates with greater Arab political maturity. The rigid radical nationalist ideologies of Nasserism and Ba'thism were either replaced or modified by more flexible approaches to international and regional developments. The "new pragmatism" was exemplified by the coordinated Syrian-Egyptian offensive against Israel in October 1973. Meanwhile, a moderate, oil-rich Saudi Arabia emerged as a decisive force in the Arab balance of power, and the Arabs were now prepared to seek American support for a comprehensive negotiated peace with Israel.

But the era of the "new pragmatism" came to an end with Anwar Sadat's visit to Jerusalem in 1977 and the subsequent Egyptian-Israeli peace treaty. The other major Arab regimes not only condemned and isolated Sadat's Egypt for breaking ranks but they also became polarized into rival alignments. Taylor does not hide his sympathy for the Iraqi-Saudi-Jordanian axis, which he sees as the heir to the "new pragmatism"; Syria and Libya form the "counter-axis."

Taylor is not far off the mark when he writes that "the pan-Arab approach has done more to impede than to promote unity" and that the Arabs would do better "to explore the possibility of regional integration" (p. 119). In fact, several Arab states have already begun to engage in regional economic planning and cooperation in the face of continued political fragmentation and polarization. This is the

only way the Arabs are likely to achieve greater unity.

Taylor's book is a concise account of the course of inter-Arab politics after World War II, which he supplements with twenty-one appendixes ranging from the controversial Palestine National Charter to the resolutions of several Arab summit conferences. Otherwise, his study leaves much to be desired. He does not provide a comprehensive framework of analysis. As a result, one would never know from his book that domestic pressures weigh heavily on the regional strategies of individual Arab states and thus on the precarious Arab balance of power. The "new pragmatism" was much more than a response to previous failures at unity and the disastrous defeat of 1967; it was a direct outcome of fierce power struggles within the Egyptian and Syrian ruling elites over which domestic and international policies to pursue. Similarly, Taylor neglects to place Sadat's peace strategy in 1977 in the wider context of his overall policy, which included greater economic and political liberalization in Egypt, a decisive rupture of relations with the Soviet Union, and an accelerated movement into the American orbit. To understand the various dislocations, shifts, and readjustments in the Arab balance of power we must explore the complex ways domestic and international forces interact in the individual states that are locked into the Arab system. Taylor has not done this, and the result is an inadequate study.

PHILIP S. KHOURY

Massachusetts Institute of Technology

AFRICA

J. DESMOND CLARK, editor. *The Cambridge History of Africa*. Volume 1, *From the Earliest Times to c. 500 B.C.*, New York: Cambridge University Press. 1982. Pp. xxiii, 1157. \$89.00.

Less than twenty years ago when the first courses in African prehistory and history were being contemplated and slowly introduced in African and American universities, a constant criticism was that the step was premature as we did not have the secondary sources for students to consult. It is a measure of the pace of research, in which J. Desmond Clark, the editor of this volume of the *Cambridge History*, has always been in the vanguard, that in less than a generation a substantive reference work of an exceptionally high quality has been produced. In a comprehensive volume of over one thousand pages of text we are taken from the geological and palaeoclimatological setting of the African continent through the early stages of development right up to the fall of the twenty-fifth dynasty (Kush) in Egypt.

A commendable regional and thematic balance is maintained throughout: the first four chapters are devoted to the palaeoecological setting by H. B. S. Cooke and Karl Butzer, the origins and evolution of the African hominidae by F. Clark-Howell, the earliest archaeological traces by Glynn Isaac, and the middle-Stone Age by Desmond Clark, and comprise 16–20 percent of the text; the chapters on Egypt by Bruce Trigger on the predynastic period, by Barry Kemp on the Old and Middle Kingdoms, and by David O'Connor on the period 1552–664 B.C. comprise 26 percent; regional surveys of the latter palaeolithic in northern Africa by Philip Smith and on sub-Saharan Africa by David Phillipson total 12 percent, while studies on the origins of indigenous African agriculture by Jack Harlan, on pastoralism and cultivation in North Africa and the Sahara by G. Camps, and on early food production in sub-Saharan Africa by Phillipson accounts for an additional 15 percent. As is usual in the Cambridge histories, the volume has a superb bibliography and bibliographic essay.

In this volume the authors have stressed many of the newer concerns of archaeology with behavior and human variability. The strong points of the book include its 180 text figures and plates and the care taken by the editor to make sure that the volume is fully up-to-date, which is quite rare in a collaborative work of this nature. A refreshing feature of the early chapters is the integration of the North African evidence. Far too many studies have treated Africa as synonymous with the area south of the Sahara or with the Black Africa of more popular books. Unlike earlier syntheses of African archaeological evidence, the authors give predominance to plans of sites. This approach reflects their collective interest in spatial relationships rather than in mere chronology. In the chapters on Egypt we learn much more about towns and the precincts of temples and tombs than about the contents of tombs.

This is a difficult book to read, for it is loaded with relevant facts. It can be faulted, however, for what is left out rather than for the coverage of what is included. Art on the whole seems to get short shrift, particularly in the chapters on prehistory. Rock art is one of Africa's greatest cultural achievements. New dates on rock art from South Africa indicate that it is as old in parts of Africa as the better known Franco-Cantabrian Ice Age art, while Saharan art, besides being unbelievably prolific, is certainly indicative of the creative and aesthetic capabilities of the once more abundant Saharan populations. Although Phillipson uses some linguistic evidence, an insufficient attempt is made in the later chapters to recognize the full potential for linguistic reconstruction and its implications for the prehistorian in the closing millennia of the pre-Christian era.

The new archaeological evidence from eastern

Africa in the early prehistoric period, especially the contributions of the Berkeley school of researchers under Clark's inspired leadership in Ethiopia, Kenya, and elsewhere in Africa, is perhaps the most important aspect of the first part of this volume. Many of the distribution maps are based on Clark's own lifelong work on the *Atlas of African Prehistory*. The impact of radiometric dating, whether potassium argon determinations for the first hominids and tool makers or radiocarbon determinations for periods after 50,000 B.P., have transformed our understanding of African prehistory and allowed a reference to a universal time scale rather than to the numerous lists of "cultures" that used to befog historians up to a generation ago. Clark is to be congratulated on providing a definitive overview of African prehistory that is at one and the same time monumental, stimulating, and exhaustive. This will certainly be an invaluable reference tool for a very long time. It will appeal not only to African historians but also to ancient historians, as it redresses the previous Asian or Eurocentric assessments of ancient Egypt by focusing on Egypt in its African time perspective. One regrets the high price, which will limit its circulation among students, but one hopes that Cambridge University Press will publish individual chapters or sections as modules at a lower price that can be revised when new material accumulates.

MERRICK POSNANSKY
University of California,
Los Angeles

MORDECHAI ABIR. *Ethiopia and the Red Sea: The Rise and Decline of the Solomonic Dynasty and Muslim-European Rivalry in the Region*. London: Frank Cass; distributed by Biblio Distribution Center, Totowa, N.J. 1980. Pp. xx, 251. \$29.50.

Mordechai Abir's book covers the history of Ethiopia from the foundation of the Solomonic dynasty in A.D. 1270 to the collapse in 1632 of the attempt of the Emperor Susinyos to force Catholicism on the Ethiopian national church and to create a European type of administration.

The peculiar character of Ethiopian politicosocial institutions is described in which the population was divided into nobles, priests, and farmers, and in which trade was conducted by Muslims—the Jabarti, mostly subjects of the Christian king. The military consisted of a small soldier class, whose origin is not discussed, slaves, and mercenaries. Farmers were forbidden to fight; they had to work and hand over most of their produce.

The direction of expansion from Showa and Amhara, the homeland of the dynasty, was originally to the south and west, where the Ethiopian kings

were in competition with the Muslim sultanates that had grown up in the Harar region and on the eastern edge of the plateau, and were expanding into the rich Gibe and Omo basins. Abir points out that the conflict with the sultanates, until the appearance of the Portuguese in the sixteenth century, was economic and not religious. It was only at that time that Ethiopia became part of the Dar al-Harb. Even then, toward the end of the century, religion ceased to be more than conventional propaganda. Mutually profitable commercial transactions were the business of life, not *jihad* or crusading.

Abir emphasizes the problem of government when administrators and soldiers could only be rewarded with land, which turned them into feudalists and probably separatists. The failure of the Solomonic kings, many no less able than their European contemporaries, to create a centralized administration and a national consciousness is ascribed to their preoccupation with the war with the sultanates, the heterogeneous composition of their subjects, and the failure of the church to undertake the evangelization of the conquered lands.

After the trauma of the conquest by Mohamed Gran (1529–43) and the overrunning of the south by the Galla pastoralists, the Ethiopian monarchs turned to the north and west. At the same time the trade routes moved from the Gulf of Aden to the Red Sea, and Massawa and Suakin developed at the expense of Zaila.

One of the merits of Abir's book is his treatment of Ethiopia's relations with the Mameluks and Turks, always important because of the necessity of an *abuna* from Egypt, the Yemen and the Hedjaz, in the context of their common situation as Red Sea states, and the northern neighbor, the Funj sultanate on the Upper Nile.

The sketch maps do not show all the places mentioned in the text, and the names of the two rivers, the Webbe Shebeli and the Juba, are misplaced.

Abir's book will be read with benefit by students of African and Arabian history and anyone interested in the Red Sea, the main line of communication between the Indian Ocean and the Mediterranean.

JAMES KIRKMAN
Cambridge, England

PATRICK MANNING. *Slavery, Colonialism, and Economic Growth in Dahomey, 1640–1960*. (African Studies Series, number 30.) New York: Cambridge University Press. 1982. Pp. xvii, 446. \$49.95.

Patrick Manning's well-known doctoral thesis of 1969 is here expanded into a more ambitious and important study. The sophistication of his methods, in respect both of economic model building and of

statistical technique, and the boldness of his interpretations are equally impressive. There is interesting new evidence from the Porto Novo archives, but the work is essentially based on careful analysis of the very extensive published sources, primary and secondary, statistical and descriptive.

Manning has most impressively consolidated, rethought, and extended his original research on the economic history of southern Dahomey, 1890–1914. By shifting emphasis from the relatively well-documented export trade to the commodity exchange sector of the southern Dahomean economy, he forms an assessment of colonial rule that will please neither apologists nor doctrinaire critics. The colonial state did not consistently serve the interests of the French bourgeoisie, but neither did it consistently serve anyone else. The gross domestic product continued to grow under colonialism until 1914 at a rate comparable to that of Great Britain; but this was “conditioned by domestic and international economic factors” (p. 222), rather than by state action. Though the gradual abolition of slavery encouraged growth, this was more than offset by fiscal policy. After Dahomey’s incorporation in *Afrique Occidentale Française* in 1905 around 3 percent of the gross domestic product was exported annually to Dakar and largely devoted to building up the federal infrastructure. The colonial state, not its citizens, benefited from improving terms of international trade (pp. 151–54). Such public investment as took place was financed principally through the tax contributions and forced labor of Dahomeans; Manning suggests that it retarded or distorted the expansion of a once vigorous indigenous capitalism. In explaining the miseries of contemporary Benin, “the replacement of growth with stagnation must be attributed primarily to the policies of the colonial state, rather than to the workings of the capitalist world economy in general” (p. 260).

This is controversial stuff, but Manning reasons powerfully. His attempts to measure changes in the domestic economy through imperfect statistics of external commerce and taxation are reinforced by a penetrating analysis of social and political development, with many revealingly illustrative examples documented. It is the earlier chapters, in which Manning’s analysis is extended back to 1640, that provide the most severe tests of his quantitative methods. Here his hypotheses can be tested only against fragmentary figures and uncontrollable samples, with admitted margins of error of 40 percent or more. Even the most imaginative and informed use of counterfactuals can hardly redress the deficiencies of surviving evidence; it is easier to establish orders of magnitude than statistical series, to refute conventionally held simplicities than to achieve precision. Manning agrees broadly with the views of the late Werner Peukert that the impor-

tance of slave exports for Dahomean society has commonly been exaggerated; neither a dynamic of capitalist development nor an all-embracing catastrophe, their evil effects might be compared to those of widespread trade in alcohol or narcotics. Yet the figures on which these two scholars base comparable conclusions differ by up to 100 percent (p. 35). The limits, besides the merits, of new economic history are illustrated in this important book.

JOHN D. HARGREAVES
University of Aberdeen

MARC MICHEL. *L'appel à l'Afrique: Contributions et réactions à l'effort de guerre en A.O.F., 1914–1919*. (Afrique, number 6.) Paris: Publications de la Sorbonne, for the Institut d'Histoire des Relations Internationales Contemporaines. 1982. Pp. ix, 533.

Anticipated in the profession for some years, Marc Michel’s ambitious doctoral thesis fulfills all the promises it held out; it is thorough, complete, and objective. The fruit of painstaking archival research in Paris, Dakar, London, and Bamako and of oral interviews with surviving *tirailleurs noirs*, the book studies the recruitment of black troops in French West Africa, their training and deployment in France during the First World War, and finally the return of the survivors to Africa.

Michel reveals that while a group of colonialists around General Mangin waxed eloquently on the virtues of recruiting African troops for a possible European war, the colonial administrators in West Africa and the highest military officials in Paris were less than enthusiastic. They were uncertain that the troops would be that crucial to the French war effort, and they feared the impact on African society of troop recruitment.

Once the war broke out, however, these hesitations were swept aside. French Governor General William Ponty offered to raise black troops, and the Senegalese deputy in the Parisian National Assembly, Blaise Diagne, also enthusiastically supported the recruitment. In addition to a strongly felt commitment to France, which he shared with Ponty, Diagne believed that a large participation of Africans in the war effort would win French gratitude and willingness to grant major political reforms overseas. The recruitment effort did not depend on volunteers; rather, young men were forcibly rounded up in their villages and transported to the coast and from there to Paris. There was considerable resistance; young men fled at the approach of recruiters, and those mustered in fled from the human convoys, which often resembled the old slave caravans. Village chiefs seem to have offered up to the recruiter the marginal individuals of the

community: the low-caste person, the infirm, and others considered of low value to the society.

Although the French colonial administration feared that the entry of Turkey on Germany's side would bring Muslim resistance to the French war effort in Africa, it was to be pleasantly surprised. All the major Muslim sects and leaders officially supported the recruitment of men and the economic mobilization of the region. In some non-Islamic regions there was violent resistance, which was brutally repressed. In the Western Volta a population of some one hundred sixty thousand people living in five hundred villages rose up in protest against the recruitment efforts. Violent resistance broke out among peoples in areas that had been relatively unaffected by previous French rule and to whom the war effort represented a quantum leap in French interference in their daily lives.

The two hundred thousand Africans who were recruited in Africa had relatively little impact on the French war effort, constituting but 1.7 percent of the French army. The war had an important impact, however, on Africa. The black troops suffered a dreadfully high casualty rate; between one-quarter and one-fifth were killed. Michel does not believe that the French command used the black troops as cannon fodder. Rather, he argues persuasively that because they were in the infantry they suffered a casualty rate akin to that of the French infantry. Michel does not spare the high command, however, in its negligence of the health, sanitation, food, and living conditions of the *tirailleurs*. The high command, subject to stereotypical views of warlike Africans, seemed to believe that the *tirailleurs* were born warriors, and very little was done to train and prepare young men from the bush for the white man's war.

The essential impact of the war effort, Michel shows, was to make the French administration more intrusive in the lives of its subjects than it had been before. With the need to recruit troops and encourage the growth of staple products, the French tightened their rule, and it was really only as a result of the war that even the most distant areas were to feel the full impact of colonial rule. So the war, rather than loosening the bonds of colonial rule, as some would argue, in fact tightened them. Only after the Second World War were the colonial ties to become unknotted.

The Sorbonne should be commended for publishing this important study. The reader's only regret is that more care was not spent on proofreading; texts and notes, particularly in English, are marred by typographical errors and omissions of whole words.

WILLIAM B. COHEN
Indiana University,
Bloomington

T. O. BEIDELMAN. *Colonial Evangelism: A Socio-Historical Study of an East African Mission at the Grassroots*. Bloomington: Indiana University Press. 1982. Pp. xix, 274. Cloth \$29.95, paper \$12.50.

T. O. Beidelman's *Colonial Evangelism* is a well-researched, aptly titled, and a delightfully produced book that, among other things, takes a penetrating look at how the Church Missionary Society (CMS) radically changed and culturally dominated Kaguru society, a relatively small community in eastern Tanzania, from about 1876 to 1958. Through a careful and systematic analysis of the Society's systems of manipulation, especially those relating to the quality of education prescribed for the Africans, the disparity between the local African and European staff, CMS's disinclination to encourage local church independence, and many others, the author makes the convincing argument that the intention of the CMS was not geared to the ultimate liberation of Kaguru. Rather, its objective was to exercise unlimited colonial domination over the native peoples. Like other missions elsewhere, CMS policy at Kaguru "aimed at overall changes in the beliefs and actions of native peoples, at colonization of heart and mind as well as body." Thus, the CMS invariably "demonstrated a more radical and morally intense commitment to *rule* than political administrators or business men" did (p. 6; emphasis added). And by radically changing the patterns of life in traditional society through religious prohibitions and condemnations, the CMS missionaries not only became "unwitting agents of destruction," but failed "to develop a new society in which Kaguru could live meaningfully" (p. 152).

In addition to establishing CMS endorsement of British imperialism, Beidelman also points out the lack of correspondence between the Christian teaching of the CMS European missionaries and their pattern of life. The contradictory image is clearly reflected in their hypocritical actions as well as in their racist behavior toward Africans. Africans were naturally scandalized and disillusioned. "Few of them [European missionaries] show by their life what they teach. If the doctrine they teach originated in Europe, I would not be a Christian" at all (pp. 190, 189).

Throughout the book the author insists that he had approached his subject from a "grassroots" perspective, which reflected the viewpoints of Europeans and Africans. But this is only partly true, for the viewpoints of contemporary European missionaries at Kaguru are conspicuously lacking in the study. Whatever the reason, this reviewer thinks the omission is regrettable as it tends to weaken the claim of even-handedness in the study.

In reading *Colonial Evangelism* this reviewer was particularly struck by the author's undisguised am-

bivalence toward enforced social change and his candid admission of inevitable bias, inasmuch as "it is neither possible nor desirable to efface personal values from useful social research" (p. xvii). For Beidelman to suggest that the iconoclastic action of colonial intruders—administrators and missionaries—is analogous to "enforced sociability and communality" in contemporary Tanzania is certainly absurd. This reviewer has the distinct impression that the author harbors considerable ambivalence toward the pattern of social engineering currently in process in Tanzania—that is, *Ujamaa*. His veiled attack on this social and economic reconstruction merits quoting in full, if only to highlight the absurdity of his position. "One of the misfortunes of the Kaguru is that for over a century powerful and aggressive strangers have tried to inflict supposedly useful and progressive changes upon them. Few if any of these supposedly well-intentioned outsiders paused to listen to what Kaguru themselves might want. In earlier periods, the proffered goal was a new, Christian Kaguru society alien both to the world the European missionaries and administrators knew at home and to that of Kaguru tradition. Now the goal takes the form of enforced sociability and communality founded on altruism harshly passed upon Kaguru by self-righteous, schoolmasterish, bureaucratic elite. Empire building has been replaced by nation building, and Europeanization by African nationalism; but the forced benevolence remains the same" (pp. 109–210, xvi). Prescription? Leave Kaguru alone; dismantle *Ujamaa* so that Kaguru society can return as "it functioned in the past." Is this not naive of a distinguished anthropologist? The *American Historical Review* may be as good a forum as any to suggest to African "experts" to give African social, economic, or political experiments a chance!

Overall, *Colonial Evangelism* is an excellent contribution to Africanist literature. It should appeal to a wider audience because of its interdisciplinary character, and students of European imperialism will certainly find it very useful, informative, and insightful. In a work almost totally free of errors (except pp. 17 and 164), it is, however, unfortunate the author should cite one of the reviewer's published works incorrectly (p. 11).

F. K. EKECHI
Kent State University

PHYLLIS LEWSEN. *John X. Merriman: Paradoxical South African Statesman*. New Haven: Yale University Press. 1982. Pp. xii, 431. \$40.00.

Any highly intelligent cultured man, who was both a perceptive observer of contemporary affairs and active in public life for more than fifty years, is a

natural candidate for a biography. Should he also happen to be a prolific writer of interesting letters, and his correspondence survive, historians are sooner or later certain to get to work. John X. Merriman, son of an Anglican clergyman who emigrated to the Cape Colony and became bishop of Grahamstown, is just such a figure. Elected to the Cape's parliament in 1869, he occasionally held office (most notably as treasurer, and, eventually, prime minister) and remained in the forefront of South African politics until 1921. He experienced, recorded, and contributed to the transformation of that country in a most turbulent period, one marked by two Anglo-Boer wars, the political union of South African states in 1910, industrialization and agricultural change consequent on the discovery of diamonds and gold, and the progressive implementation of white inclinations to control and subjugate the much larger nonwhite population.

This is more than a fine portrait of a striking individual, however. Many scholars are acquainted with Merriman's papers, preserved in Cape Town. More still will know Phyllis Lewsen's selections from Merriman's correspondence, published in the 1960s, and her 1971 article in *Race* on the fragile reality of the Cape's liberal political tradition, which together provide much of the raw material and the interpretative framework underpinning this book. They may feel they know their Merriman, and, with others sharing the fashionably dismissive view of biography, push the book aside: but all such temptation should be resisted.

This book not only portrays with great sensitivity Merriman's own continuous enlightenment and growing humanity, but also skillfully conveys the problems of imperial relations and colonial politics. It is also an essay on the transference of Victorian values to a new environment and a study of the halting evolution of a communal liberal consciousness in unpromising circumstances. Others have seen the liberalism of the Cape, which Merriman represented, as necessarily weak and temporary, rooted in the material conditions of a limited commercial society and briefly supported by contacts with metropolitan Britain. This book agrees that the Union of 1910 marked the creation of an industrializing state in which the Cape's liberalism was rapidly overwhelmed, but rejects any suggestion that this was inevitable. Merriman's career is seen as providing evidence of a continuing vitality and flexibility in the Cape's outlook, sufficient for more constructive responses to the problems of color and class in modern South Africa than anything else yet seen. Merriman's exclusion from the premiership of the new union was not just a personal tragedy, but represented a far greater collective failure of political leadership and vision that has had dire consequences. More broadly still, the book provides a

telling commentary on the perennial problems of liberals in politics.

There are some uneven patches, very probably the result of its being unavoidably "a shortened version of a very much fuller work." Occasionally the author's sympathy with Merriman seems to inhibit scrutiny or to exaggerate his claims to influence. Nevertheless, this remains a subtle, passionate, and distinguished historical study.

ANDREW PORTER
King's College
London University

JOHN WESTERN. *Outcast Cape Town*. Minneapolis: University of Minnesota Press. 1981. Pp. xvi, 372. \$22.50.

"As a social geographer, I pursued research in Cape Town and then wrote an interpretative report on it," states John Western at the start of his concluding chapter. Western is too modest about his "report." It does, indeed, document the relentless application of residential apartheid to South Africa's previously most racially integrated city and its devastating impact upon coloreds, about one hundred thousand of whom were forced to move under the terms of the Group Areas Act passed in 1950. It additionally provides convincing evidence to expand understanding of the attitudinal shift among younger coloreds toward "black consciousness" and identification with the African majority. Yet it has even greater value in illuminating the dynamics of the apartheid system as a whole through its examination of one segment of the population in one South African city.

Residing in Cape Town from 1974 to 1976, Western gleaned local newspapers, government reports and records at all levels, and made contacts with individuals and organizations long involved in colored life. His most distinctive data, however, were derived from interviews with members of one hundred households removed from a colored "pocket" and scattered throughout the Cape Town area to both new and old residential districts officially designated for coloreds. By interweaving data from his diverse sources Western shows how the Group Areas Act has operated as "an instrument for institutionalizing the disadvantage of those not in power" (p. 234) by limiting personal interracial contacts primarily to the marketplace and government offices, by "cleansing" the historic center of Afrikanerdom's "Mother City," and by eliminating business competition, particularly from Indians. In the process white real estate interests and new homeowners, mostly upper-class professionals (many of whom, ironically, support the opposition Progressive Federal party), had benefited materially

from the "gentrification" of the areas vacated by coloreds. Simultaneously, once relatively cohesive colored communities have been fragmented into groups and individuals characterized by growing "apprehensiveness, distress, and disadvantage" (chap. 8). Yet, paradoxically, Western finds revealing indications of a heightened sense of self-identification based on race superseding a previously strong sense of community based on place.

In his penultimate chapter, "The Shantytowns: Trying to Keep the Lid On," Western focuses on not only colored squatter settlements that sprung up outside government planned townships, but "illegal" African settlements, including Crossroads. In his analysis of these phenomena and government attempts at "containment" of the coloreds and "expunction" of the Africans, Western argues that recent "liberalization" of apartheid has not fundamentally altered "a context of total dominance and one-way dependency" (p. 308).

Western's book is informed throughout by concepts and literature drawn from his discipline (chap. 4, "Social Engineering through Spatial Manipulation: The Ideal Apartheid City," seems especially directed to geographers). Yet his analysis is equally characterized by solid historical knowledge, scrupulous marshaling of pertinent evidence, and imaginative use of literary sources to highlight that evidence. In his first book Western has masterfully transformed a doctoral dissertation into an engaging and well-written study to be read with both profit and pleasure by historians, social scientists from all disciplines, and anyone concerned about South Africa.

SHERIDAN JOHNS
Duke University

ASIA AND THE EAST

JEROME CH'EN. *State Economic Policies of the Ch'ing Government, 1840-1895*. (The Modern Chinese Economy.) New York: Garland. 1980. Pp. 216.

When this monograph was first completed as a doctoral dissertation in about 1956, Jerome Ch'en was most pioneering in trying to debunk the notion that nineteenth-century Chinese political leaders were all Confucian scholars sharing a common set of orthodox Confucian values. It was assumed at that time that during this crucial period of Sino-Western contact a monolithic ruling establishment refused to acknowledge a world in tumultuous change—in inter-state relationships, in economic power, and in technological advances. China's failure to modernize was therefore due to the Confucianists' inability to meet the requirements of a modern society.

However, scores of detailed studies have since

been published on the variegated nature of Confucian statecraft in late imperial China and the diverse temperament of the men who practiced it. In the introduction to this edition written in 1980, Ch'en readily acknowledges this and cites among others the works of Lloyd Eastman, Mary Wright, Andrew Nathan, and Immanuel Hsu for their contributions on this subject. It is a measure of the author's considerable scholarly prescience and acumen that, apart from some minor revision of text and new footnotes to take into account these more recent publications, his original thesis and much of the detailed analysis have stood the test of time. Nonetheless, it is a dated book in the sense that so many of its arguments are already familiar to its readers. Moreover, in spite of Ch'en's meticulous use of the documents then available, including the eight-volume *Yang-wu yun-tung*, a new avalanche of state papers has just become available to scholars at the recently opened Ming-Ch'ing Archives of the Palace Museum in Peking. This collection, holding a staggering eight or nine million documentary items from central government records of the Ch'ing dynasty (1644–1911), is being catalogued. Some are beginning to be published.

Within these limitations, Ch'en's volume provides a masterful analysis. It gives a background description of institutional and intellectual changes, thus making it easier to understand why certain types of economic-policy proposals were made and adopted. The author follows a chronological and thematic format, dividing the nineteenth century into four periods and discussing each period by devoting sections to international trade, fiscal policy, and self-strengthening projects. From his accounts, one is struck by the crucial impact foreign trade had on so many major aspects of late imperial China. Before 1830, it contributed to a huge influx of silver (some five hundred million Mexican silver dollars or about 60 to 70 percent of China's total silver holdings by 1830). In the mid-nineteenth century, it was responsible for a scarcity of silver, which forced its price to go up in relation to copper, thus causing great misery to almost everyone, since taxes were paid in silver while the cash holdings of most Chinese consisted of copper coins. Then from the late 1860s to about 1895, receipts from maritime customs provided the major source of funds for China's various self-strengthening projects.

In his conclusion, Ch'en argues that modern economic development failed not because of the Confucian officials' corruption, nepotism, or negligence, but because of their lack of overall conceptualization, scientific thinking, and coordinated planning. Another problem was the tendency of regional administrators to compete among themselves and with the central authorities for funds and power. For example, while there were some sixteen

small and four large munitions factories in various parts of China, the lack of unity among the political authorities resulted in the factories producing completely different, noninterchangeable types of fire-arms and ammunition. Such inadequate coordination leads one to question whether there were indeed "state economic policies" as the author suggests in his title.

WELLINGTON K. K. CHAN
Occidental College

ROBERT L. IRICK. *Ch'ing Policy toward the Coolie Trade, 1847–1878*. (Asian Library Series, number 18.) San Francisco: Chinese Materials Center. 1982. Pp. xviii, 452. \$29.75.

If the crimps did not kidnap them outright, they lured them with gifts or tales of the gold mines of California—false tales, as California would not be their destination. Once in the barracoons of Macao or some other south China port, the unfortunates were watched carefully, beaten if they tried to escape, duped or terrified into signing labor contracts, then flung unceremoniously into vessels of dubious registry and even more dubious condition. Crowded tier upon tier beneath closely guarded hatches, flogged on the slightest pretext (and occasionally on none, to encourage the others), ill-fed and scurvy-ridden, many could not take the four-to-six-months' passage; some tried by arson, mutiny, or suicide deliberately to cut it short. The survivors, landed at last at Callao or Havana, were sold in labor markets hungry to replace the Indians and blacks of a previous age—and never left Peru or Cuba. This was the Pacific coolie trade of the mid-nineteenth century, as foul a business as the Atlantic slave traffic that had preceded it. And almost as difficult to stop.

Conventional wisdom holds that in the end it was the foreigner who stopped it, Peking caring not at all what happened to unwilling emigrants. Robert L. Irick will have none of this. Already driven, I suspect, by an itch to demonstrate that much for which Westerners claim credit actually lies with the Chinese, he began more than twenty years ago to look into this particular question. Almost at once, however, he was drawn away to develop the Chinese Materials and Research Aids Service Center at Taipei. He did not complete his Harvard PhD until 1971; his continued preoccupation with CMRASC prevented his doing anything to publish his thesis; and it appears now essentially unrevised. So this is not a polished book. Indeed, it shows quite unnecessary slips in editing (or word processing?). But the argument and the argument's supporting evidence more than make amends for these trivial faults and this long delay.

The argument is that, while Peking did indeed

hesitate to move against the coolie trade on China's coast and did indeed hesitate to post envoys to protect the emigrants in the places of their servitude, it did so not because it did not care but because (as with the opium traffic earlier) to regulate an evil is to acknowledge it and make it lawful. When Peking did at last move (much encouraged, Irick admits, by certain Americans) and did at last post, it did so with genuine concern, some ingenuity, and considerable vigor. Irick follows this process as much as possible from the Chinese side, using Chinese sources—this is easily the book's chief novelty and chief attraction. We learn what governors-general Lao Ch'ung-kuang and Jui-lin thought and did. China's difficulties with incorrigible Portuguese Macao were never clearer. There are moments when documentation and detail threaten to drown argument and reader together. And Irick's enthusiasm carries him a bit far: can an "almost equal" treaty with insignificant Peru really be of such transcendent importance? This is, nevertheless, a persuasive, pioneering study. John Fairbank was right to encourage its publication.

PETER WARD FAY

California Institute of Technology

R. KEITH SCHOPPA. *Chinese Elites and Political Change: Zhejiang Province in the Early Twentieth Century*. (Harvard East Asian Series, number 96.) Cambridge, Mass.: Harvard University Press. 1982. Pp. viii, 280. \$30.00.

This is a study of the political dimensions of elite social organization within Zhejiang province from the last years of the Qing dynasty until Nationalist armies invaded in 1927. R. Keith Schoppa uses a core-periphery framework and concepts of social ecology to explain political developments in terms of economic and social systems. For this purpose he divided the province into four zones ranging from inner-core to outer-periphery, examining the different compositions of their elite societies, organizational levels, degrees of politicization, and elite relations with officials or with elites of other zones. An impressive amount of detail is organized within this framework.

Schoppa has undertaken an ambitious task. Sources are numerous but fragmentary. Data must be painstakingly pieced together and generalizations derived from incomplete detail. Some statistical tables are compiled from small samples or else contain a high percentage of unknowns. These familiar difficulties of Chinese social history are openly confronted. The methodology is explained in notes and appendixes, and ways are found to support a systematic approach. One can quarrel with details while still being impressed by the thor-

oughness and honesty of the scholarship. The book sets high standards for a new kind of research in Chinese history.

This type of core-periphery analysis can become unduly static and mechanical. On the whole, Schoppa avoids this danger. He gives the most space to the dynamic, active inner-core elites and varies categories of analysis to fit different zones. Elite composition and organization changed, and politicization spread outward from the core districts during the twenty years covered. The second part of the book explores evolving interaction between elites of different zones, competing subregional alliances, and shifting relations with governmental power.

Schoppa does not generally raise broader political issues, except briefly in the conclusion. However, there is much information in the book of significance to an understanding of other parts of China and larger questions of state-societal relations. At the very least it should convince historians of China that they ought not to make social or political generalizations without considering core-peripheral differences and native-place affiliations. We now have a description of elites from a province partly within the most highly urbanized macroregion in China, and the kinds of questions Schoppa asks suggest approaches to other areas.

We also learn much about the political potential and limitations of a Chinese bourgeoisie. Unfortunately we are told little about the Shanghai business connections crucial to the dynamism of inner-zone elites of the five northern prefectures, but their political activism and economic dominance in Zhejiang is clear. However, even during a generally prosperous, stable period of relative state weakness, their political ambitions were checked by military power controlled by elites of more peripheral prefectures within the province. Eventually, the Nationalists came to power in Zhejiang in alliance with socially conservative outer-zone elites who were antagonistic to the wealthier, more urbanized, more reformist elites to the north. Such insights make this book well worth reading.

MARY BACKUS RANKIN

Washington, D.C.

PAUL HENG-CHAO CH'EN. *The Formation of the Early Meiji Legal Order: The Japanese Code of 1871 and Its Chinese Foundation*. (London Oriental Series, volume 35.) New York: Oxford University Press. 1981. Pp. xxii, 204.

In the years after 1882, the new Japanese government adopted French and German legal ideas and practices as models for its civil, criminal, and constitutional law. But between 1868 and 1882, writes Paul Heng-chao Ch'en, "the Meiji codification

movement found its major inspiration in the legal tradition of China" (p. 3). Ch'en's book offers the first extended treatment in English of the Chinese influence on early Meiji code-making. It includes a foreword by W. G. Beasley; a brief preface and introduction; three content chapters (76 pages); a translation of the *Shinritsu kōryō* of 1871 (102 pages); and a bibliography, glossary, and index.

The *Shinritsu kōryō* was one of four legal texts produced between 1867 and 1881. Only three took effect. Ch'en's choice of the second to translate is a sensible one. It is, he writes, "the first substantial code" after 1868 (p. 15). Chapter one provides the textual setting; chapter two compares the penal provisions of the various texts; and chapter three departs from textual analysis to portray judicial arrangements and the growth of the guild of those involved in dealing with the codes.

The translation itself is workable but far from flawless. Ch'en translates *Shinritsu kōryō* as "the essence of the new code," but "digest" or "concise principles" sounds far more code-like. Ch'en decided to translate "as closely as possible in the same style and wording" of the English translation, done in 1810, of the Chinese code on which the *Shinritsu kōryō* is modeled. This decision makes for awkwardness: "All accessories to the plot who also contributed to the perpetration of the premeditated homicide by striking the victim" (p. 131) is closer to the 1810 translation than to the Japanese text. Elsewhere, too, Ch'en's translation is wordy. Ch'en translates one phrase as "any person who fails to dismount from his horse but rides through a site marked by a notice board on a location where one is required to dismount" (p. 112) when the Japanese is much terser and clearer: "any person who approaches and passes a sign to dismount without dismounting."

There are also some simple mistakes. Ch'en translates an early section on decapitation: "The head shall be destroyed after display for three days, and it is for extremely cruel offenses that such a waiting period is required" (p. 84). The Japanese states: "the head shall be discarded after three days. This punishment [exposing the head] is reserved for extremely cruel offenses." Again, Ch'en translates a passage on multiple offenses: "When any person, after having been charged with an offence, commits a second offence prior to the infliction of the punishment arising from the former" (pp. 92–93). The Japanese reads: "Anyone who earlier committed a crime, has already been detected, but on whom sentence has not yet been passed, who commits a separate crime."

Flawed though it is, the translation is a contribution. The legal scholar or other interested person who does not read Japanese can now work with the code of 1871. Still, within a very few years Japan

swung from this Chinese model to Western models, and the code translated here lost out, forfeiting not merely its force of law but also any significant influence on the later codes.

RICHARD H. MINEAR
University of Massachusetts

WILLIAM MILES FLETCHER III. *The Search for a New Order: Intellectuals and Fascism in Prewar Japan*. Chapel Hill: University of North Carolina Press. 1982. Pp. x, 226. \$24.00.

Japanese preparation for total war began without a plan. This book describes how three intellectuals helped devise a plan out of the eclectic mixture of ideas that marked Japanese thought from 1920 to 1940. The three men—Rōyama Masamichi, Miki Kiyoshi, and Ryū Shintarō—appear as exemplars of a drive to harness nationalism and economic growth to achieve a greater Japan.

William Miles Fletcher III labels the views of these thinkers "fascist." Left-leaning though they were, they needed no apostasy (*tenkō*) to negate their youthful convictions. For them fascism itself was "propelled by nationalism" (p. 54) and presented a way to attain multiple goals. One goal was to exploit the emotional force of nationalism to mobilize the Japanese economy under state control for the purpose of expansion in Asia. Another goal was to justify radical reform of Japanese society as a means of diminishing the divisive effects of industrialization and restoring a sense of traditional community (*Gemeinschaft, kyōdōtai*).

The author wrote this book to answer recurring questions: Why did fascist thought appeal to a wide spectrum of Japanese intellectuals? Why did they offer so little resistance to government policies that led to war? The answers are clear enough, at least for Rōyama, Miki, and Ryū. Their personal quest for influence and their belief in Japan's destiny to lead a resurgent Asia kept them in tune with the strategy of every Japanese administration in the 1930s.

The Shōwa Research Association (1933–40) proved an ideal vehicle for promoting the aims of these three thinkers. The SRA was "a brain trust for Prince Konoe Fumimaro," three times prime minister between 1937 and 1941 (p. 3). Rōyama was the SRA's political theoretician, Miki its philosopher, and Ryū its chief economic planner. Through the SRA and their friendship with Konoe, they gained "the passing illusion of power and influence" (p. 155). But Konoe was a politician who wore ideas like clothing and changed them to suit the needs of his powerbrokering. So the fascist program dissolved with the SRA itself when the Imperial Rule Assistance Association was formed in 1940. Japan's gov-

ernment never adopted the SRA's radical program: it never became fascist. And the three intellectuals slid into obscurity as Japan's war in China and the Pacific region intensified.

Rōyama, Miki, and Ryū were prolific authors, and Fletcher excavates all of their writings and other SRA papers. The book thus features an engaging interplay between thought and action, between perceived intentions and political outcomes. Fletcher finds it useful to raise the old question of applying the concept of "fascism" to Japanese thought, in this case that of SRA members. Whether or not they qualify as fascists—they advocated no *Führerprinzip* and called for no *Gleichschaltung*—they did go further than civil and military authorities toward conceiving a "new order" within Japan. Others had urged that Japan generate an Asian renaissance, but ideas of the corporate state and economic regimentation received more systematic expression in the hands of the SRA.

The Search for a New Order is a crisp and intelligent book. It joins studies by Gordon Berger, James Crowley, Ben-Ami Shillony, and David Titus that illuminate dimensions of the 1930s in Japan. Like them, Fletcher shows us a rapidly changing society, whose elites could only grasp at solutions in the face of mounting domestic and foreign problems. He succeeds in capturing something of the anguish and promise of a decade that Japanese leaders and intellectuals ultimately failed to redeem.

GEORGE MACKLIN WILSON
Indiana University,
Bloomington

THOMAS R. H. HAVENS. *Artist and Patron in Postwar Japan: Dance, Music, Theater, and the Visual Arts, 1955–1980*. Princeton: Princeton University Press. 1982. Pp. ix, 324. \$25.00.

In this book Thomas R. H. Havens takes up the role of the arts in postwar Japan. Under such headings as "Art for Society's Sake," "Poverty of Patrons," "Arts and the State," and "Arts to the People," he explores various aspects of the relationship between contemporary Japanese culture and the public, the government, and private or corporate patrons. His main emphasis is on dance, theater, music, and painting. The discussion is based on Japanese sources as well as interviews with artists and art administrators. The text of some two hundred fifty pages is supplemented by fifty pages of notes and bibliography. What emerges is a very complete picture of the artistic scene in Japan today, presented in an interesting, well-written manner. As the author points out, a huge audience for all kinds of artistic expression exists in Japan today. The figures he cites are indeed staggering. The Mona Lisa for

example was seen by one and a half million people, while the Tutankhamen exhibit had more visitors in Tokyo than in New York. There are fifteen hundred art galleries in Tokyo, seven hundred thousand pupils of the classical ballet, eight hundred thousand students of modern dance, and over a million of classical Japanese dance. The ticket-sales figures for major cultural events are extraordinary. Particularly popular are performing artists from foreign countries, such as Martha Graham, Herbert von Karajan, and the Russian ballet. Loan shows of modern French painters, notably Van Gogh and Picasso, draw record crowds and create a sensation. Yet at the same time, as the author points out, relatively few of the hundreds of thousands of people engaged in the arts can make a living by their art alone. Of the thirty thousand professional painters only the three hundred top names can exist by selling their work; the rest must teach or seek outside employment. The same holds for musicians, dancers, and actors. But since every middle-class family wishes to have its children, especially its daughters, learn to play an instrument, dance gracefully, or pursue one of the many other polite accomplishments, literally millions of pupils exist for the many who wish to be teachers. This master-pupil relationship plays a great role in Japanese society. Just as in feudal times, loyalty is owed to one's teacher, fees are paid, gifts are presented, and tickets bought.

All of this is dealt with in a very thorough and scholarly way by Havens. What this reviewer found disappointing, however, is that the approach to the material is purely sociological, with questions of artistic quality and creativity hardly mentioned. It is true that we learn the annual income of Japan's best-known painter Umehara Ryuzaburo, but nothing is said about the aesthetic merit of his work. We are told about the difficulties of the print makers but not about the great and internationally recognized contribution of Munakata Shiko. Perhaps this lack of concern with artistic quality explains why pottery, architecture, and film—the areas in which Japan has made its greatest artistic contribution—are not mentioned. If foreign musicians sell more tickets than domestic ones, would it not also be interesting to be told if they indeed are any better. It would appear, furthermore, that Havens tends to make use of interviews with disgruntled artists without examining their creative contributions. But despite these limitations this is an excellent book and should become a standard in its field.

HUGO MUNSTERBERG
Bard College

JAPAN RAYCHAUDHURI and IRFAN HABIB, editors. *The Cambridge Economic History of India*. Volume 1,

C. 1200–c. 1750. New York: Cambridge University Press. 1982. Pp. xvi, 543. \$79.50.

Following a brief introduction (pp. 1–14), the present volume is divided into two parts, spanning ca. 1200 to ca. 1500 and ca. 1500 to ca. 1750. Spatially, it seeks to cover the entire subcontinent. The themes in part 1 include: economic conditions before 1200, the agrarian economy, nonagricultural production and the urban economy, currency system, Vijayanagara ca. 1350–1565, and maritime trade. Part 2 considers population, the state and the economy, the system of agricultural production, agrarian relations and land revenue, nonagricultural production, inland trade, the monetary system and prices, foreign trade, towns and cities, standard of living, and (in an appendix) the medieval economy of Assam.

The best part of this monumental volume is the study on oceanic trade and the economy of medieval Assam, a terra incognita so far. In his systematic and analytical study of European trade with India (1497–1750), K. N. Chaudhuri focuses on the consistent interest of the English East India Company in the textile trading network of the country. As textiles were the premier industry of the age, it requires a deeper probe if this company's endurance, in contrast with the far briefer spans of prosperity of its rivals, was not to a considerable extent the result of other factors besides its involvement in this particular commodity.

Regrettably one notes that the chapters on the economy of the Sultanate and Mughal empire will need to be used with caution because, notwithstanding the availability of a huge corpus of indigenous literature, the contributors have drawn almost exclusively upon English sources and some selected secondary works. Consequently, the editors exhibit strange notions of the economy under consideration. For example, overlooking the economic and administrative compulsions of a vast centralized state, they attribute the rulers' attempts to foster urban concentrations merely to their preference for an urban lifestyle (p. xiii). Again, in the industrial sector, forgetting the contributions of the thirteenth and fourteenth centuries, the seventeenth-century boom, and the role played by cotton textiles, they view the era as characterized by "the growth of a small range of new manufactures; such as paper and lime mortar." They also underplay the significance of the "introduction of some new technology" (p. xiii). Or again, one wonders in what manner was the economy under the great Mughals "qualitatively different" from that of its predecessors. These and similar other inadequacies have resulted in the failure of the editors to integrate the different aspects of the imperial economy into a cohesive

whole. In fact the emerging picture is so blurred as to make it impossible to decipher the contours of the economic order of the period.

Taking Irfan Habib's papers (pp. 48–93) to illustrate, the sultanate era is unexpectedly rich in source material, especially when supplemented with Tibbi works and the series of lexicons citing some of the craft tools and equipment required. Habib is evidently unaware of this set of literature; but, more surprisingly, he has not fully utilized even the well-known ones, such as *Ejaz-i-Khusruvi* of Amir Khusru. Thus the sultans' basic economic policy of maximizing agricultural and industrial production is lost on him. He is therefore unable to link the thirteenth century's recorded attempts at reclamation of land with the introduction of the fourteenth-century canal system. Similarly, he overlooks the urban-based dignitary *amir-i-kohi* and the enlistment of the rural functionary *Mafruziyan*, who together worked for the extension of land under cultivation. Viewed in this context, land grants to the learned and divines (p. 76) may be seen as the state's endeavor to use even these nonproductive elements to help bring additional land under plough. Further on, by ignoring the available price quotations for 1325–51, 1351–88, and ca. 1500, (p. 51), Habib has missed the prime extant index to assess the fluctuations in the aggregate supply of foodgrains, especially the phenomenon of overproduction and the ensuing glut in imperial markets after 1351. Finally, while appropriation of the agricultural surplus from the small landholders by the ruling class is characteristic of any feudal society, Habib's elaboration of the system under the sultans of Delhi (pp. 60–76) bypasses the key question: how far were these overtaxed peasants, individually or collectively, able to help or hinder the constructive forces of the day?

Raychaudhuri, in his treatment of nonagricultural production in the Mughal empire, contributes his own share of shortcomings. Although caste restriction stood in direct proportion to the job potentiality curve, which fluctuated constantly, he maintains that there was only limited occupational mobility for the entire period from 1500 to ca. 1750 (p. 285). As well as this disregard for the time factor, he also exhibits inconsistencies, such as in his discussion of technology (pp. 291–95), or naive, sweeping generalizations. Against all evidence to the contrary, he insists that villages were the principal manufacturing centers (p. 279).

The section on the currency system of the sultans (pp. 93–101) is informative and useful. The maps are good. The volume reads very well. Bibliographical entries could all have been made in simpler alphabetical order.

HAMIDA KHATOON NAQVI
New Delhi, India

N. GERALD BARRIER, editor. *The Census in British India: New Perspectives*. New Delhi: Manohar; distributed by South Asia Books, Columbia, Mo. 1981. Pp. xiv, 234. \$17.50.

The decennial census of India, begun in 1871, contains thousands of volumes of social, economic, and demographic data whose scholarly use has been increasing in recent years. Although no scholar can comprehend the entire collection, many have dipped into it for specific studies. This volume grew out of a conference on the use of the census and ways to compensate for its biases and inaccuracies. Of its eight chapters, three discuss the impact of the census itself on Indian history, and five provide case studies showing how the census can serve as a historical source. Some contributions are weightier than others, but the value of any particular paper depends on the research interests of the reader.

As N. Gerald Barrier, the editor, points out in his introduction, the census is not only a collection of statistical tables but also a very human document, whose narrative sections and handbooks tell of the adventures of the census-takers—narratives with which the researcher must be familiar in order to understand the statistics. Joseph Schwartzberg's contribution, "Sources and Types of Census Error," explains why the research scholar must approach the data with common sense and healthy skepticism: at the top officials manipulated categories for political purposes; at the bottom underpaid and poorly educated enumerators often misunderstood their tasks, while their respondents, through fear or craft, frequently provided false information.

Richard Martin provides a useful annotated bibliography on the census. Kenneth Jones's interesting article, "Religious Identity and the Indian Census," shows the impact of the census itself on communal identity. The nineteenth-century British, preoccupied with Indian religions, used religion as their principal organizing category. This is turn changed Indian conceptualization of religion, and each census became a "scoreboard" indicating the progress of one's own religious community. By heightening communal identity, the census changed the course of Indian history. Frank Conlon shows the similar role of "caste," as well as the way in which the shifting of caste categories from one census to another makes comparisons over time treacherous. G. A. Oddie uses the census to draw a profile of two South Indian, Christian communities. By ingenious statistical inference and topographical mapping, Harry Blair uses caste data from the 1911 census to study correlations of caste with voting preference in Bihar in the 1960s. Charles Newell employs sophisticated computational refinements and even recalculates census figures to trace changes in the urban

labor force in twentieth-century Tamilnad. J. Daniel Moore's study of plantation labor in Mysore from 1871 to 1941 provides a model for using the census to trace internal migration.

The book is an invaluable guide to the census, its resources, pitfalls, and techniques to overcome these pitfalls. The arrangement of chapters could have been more logical and the papers less repetitious, but this is a pioneering effort. Any minor deficiencies are far overshadowed by the importance of the book for teaching and research in modern Indian history.

BLAIR B. KLING
University of Illinois,
Urbana-Champaign

GAIL MINAULT. *The Khilafat Movement: Religious Symbolism and Political Mobilization in India*. (Studies in Oriental Culture, number 16.) New York: Columbia University Press. 1982. Pp. 294. \$25.00.

It is a rarely appreciated fact of modern South Asian history that Indian Muslims triggered the first nationwide political agitation of the nationalist period, the noncooperation Khilafat campaign. The campaign, which linked Hindus and Muslims, began in 1919, when a group of Muslim political and religious leaders organized to protest the British government's role in the dismemberment of the Ottoman empire and the threat which the dissolution seemed to pose to the role of the Ottoman Khalifa, or Caliph, the symbolic leader of the Sunni Muslim community. Yet, while Muslim leaders ostensibly concerned themselves with Ottoman and pan-Islamic issues, they were in actuality preoccupied with the fate of the Islamic community of India, one which was not only subordinate to the British but which also feared that it would be overshadowed and outvoted by the numerically superior Hindu community in an independent Indian state. In *The Khilafat Movement* Gail Minault is concerned with just this internal focus of Indian Muslim concerns, and she utilizes both English and Urdu sources to compile a detailed study of this "campaign by a particular group of Indian Muslim leaders to unite their community politically by means of religious and cultural symbols meaningful to all strata of that community" (p. 2). Her book is the most complete study of the Khilafat movement yet published; and it has been carefully printed by Columbia University Press, whose editors have enriched the volume with photographs of Khilafat leaders and delicate filigrees of evocative Urdu verse.

Gail Minault is one of a growing group of scholars who have gone beyond English-language archives to study Indo-Muslim society during the nationalist period through the medium of its own language. As

a result, she is sensitive to the critical importance that religious and cultural values had in shaping the outlook and political decisions of Khilafat leaders. She is also acutely aware that differences among these leaders often reflected the diverse constituencies they represented. These ideas are best articulated in the first chapter, "On the Emergence of the Leadership." There Minault demonstrates her close intellectual association with David Lelyveld, who has written on the modernist Aligarh Muslim University, and Barbara Metcalf, the author of a new book on the traditional Deoband religious college, by stressing the importance of Muslim educational institutions as training grounds for Khilafat leaders. She is able to show how Aligarh graduates and disciples of traditional religious leaders came together over the Khilafat issue, even while the university graduates might be more concerned with the nationalist cause than with religious revival. Indeed, these themes, which appear in this chapter and recur frequently in her narrative history of the Khilafat movement until its collapse in 1924, distinguish this account of the Khilafat campaign from several earlier versions. That is, it is her sophisticated perspective on Indo-Muslim society that has led her to depict its values, institutions, and internal distinctions, rather than any major new information about political events, which makes this a useful book.

As is true of most studies of Indian nationalist campaigns, this book leaves much unsaid—omissions which may be impossible to correct until more regional studies become available. Thus, for example, although Minault stresses the heterogeneity of Indian Muslims she does not attempt to describe the cultural and linguistic diversity of the population or suggest how the predominantly Urdu idiom of the Khilafat leaders may have limited their effectiveness in winning support among such groups as the Tamil-speaking Muslims of Madras. Nor does she give the reader a good sense of how widespread the Khilafat movement was. Like other books on the noncooperation campaign that describe it as a "mass movement," it is difficult to determine from this study the social depth and geographic breadth of this mass support. How many Muslim peasants took part, apart from the unique case of the Mappila rebellion? Were Muslims in the princely states of Kashmir and Hyderabad immune to the Khilafat excitement? The historiography of the Khilafat movement now needs local studies, not only to provide data but also to impart a sense of how local Khilafat committees arose, functioned, and collapsed. It would also be useful to have estimates of the long-term effects which the movement had on Indian Muslim consciousness and political leadership. Did Khilafatists become organizers for Jinnah

and the Muslim League in the 1930s? It is nearly always assumed that the Khilafat movement did exert a substantial effect on later Indo-Muslim politics; but, apart from the biographies of a relatively few leaders, little is known about the movement's legacy in the twentieth century.

STEPHEN F. DALE
Ohio State University

ROBERT GRANT IRVING. *Indian Summer: Lutyens, Baker, and Imperial Delhi*. New Haven: Yale University Press. 1981. Pp. x, 406. \$39.95.

On March 13, 1912, the future Sir Edwin Lutyens wrote ecstatically to the future Sir Herbert Baker: "Delhi is all right!! I start on 27 March!!! It is a wonderful chance." Indeed it was. Since 1450 B.C. there had been fifteen cities of Delhi. New Delhi—Imperial Delhi—was to be greater than any of these: a mighty symbol of the greatest empire since ancient Rome. Years before, in *Tancred*, Benjamin Disraeli had envisaged the transfer of the capital of the British empire from London to the heart of India. At the Coronation Durbar of 1911, that dream seemed almost about to come true: King George V announced the removal of the capital of India from Calcutta to Delhi, and the establishment of a newly founded imperial city. The Durbar itself had been an occasion of more than imperial splendor. The editor of the *Times* remembered it as "probably the most magnificent and dazzling spectacle of its kind that the eye of mortal has ever beheld." New Delhi was to be more splendid still. As the king's secretary, Lord Stamfordham, put it: "We must . . . let [the native] see . . . the power of Western science, art, and civilization." "Like Rome," wrote Sir George Birdwood, Imperial Delhi "must . . . be built for eternity."

The building of New Delhi—its political and social context, its aesthetic dilemmas, the logistics of its construction—forms the theme of Robert Grant Irving's *Indian Summer*, an impressive work based on a relentless mastery of primary sources, superbly illustrated, and written with an enthusiasm and dash appropriate to its subject.

Well, what style was the new city to take? King George V favored native forms, Mughal for preference. So did pro-Indian pundits and scholars like E. B. Havell. But the bulk of English architectural opinion was in no doubt that some form of classicism was essential. The *British Architect* argued for Italian, as did the *Times*; the *Architects' and Builders' Journal* for Greek; and the *Builder* for English Renaissance—Britain's "natural means of expression." To arts and crafts architects like C. F. A. Voysey, however, such arguments were aesthetic

"claptrap." And to Indian nationalists the imposition of any European style was nothing less than an insult. Some form of compromise between East and West was therefore inevitable. The viceroy, Lord Hardinge, advocated a mixture of Palladian and Pathan principles, a blend of Indian symbolism and Renaissance composition. So did Lutyens's principal collaborator, Herbert Baker. And so did Hardinge's predecessor, Lord Curzon: he cited the fusion of Moorish and Renaissance elements in Southern Spain. Lutyens himself had a profound contempt for native Indian architecture, whether Mughal or Hindu. Equally deplorable, he believed, was the hybrid Eurasian style—"half-caste architecture"—of the eighteenth-century and nineteenth-century colonial buildings in Bombay or Madras. Still worse were the prefabricated, Jacobethan absurdities of Simla. Something subtler was needed, something nobler, something worthy of George V's conception of the "union and fusion" of Indo-European culture. Lutyens found it in an architectural synthesis of his own invention: an Anglo-Indian classicism that drew inspiration from European and Indian traditions without copying either. This was not an occasion for "fancy dress," he explained; he set out to "build as an Englishman dressed for the climate," conscious only that his tailor hailed from the Punjab rather than from Saville Row. The result was eclectic certainly, but eclecticism purged and abstracted, purified by its architect's passion for reductive geometry.

Lutyens's architectural coadjutor and sparring partner, Herbert Baker, was seized by a less cerebral version of the same dream. His turn of mind—and hence his architectural aesthetic—was literary rather than abstract. Hence his delight in symbolic associations, his commitment to traditional features, whether Renaissance or Mughal. Hence, too, his inability to match that sublime fusion of forms that was Lutyens's special genius.

The ceremonial inauguration of New Delhi in 1931 was more of a requiem than a baptism. The cost had soared to more than £10 million. And the optimism of the Edwardian empire had gone for good. Indian nationalism and the Great War had altered the political climate irreparably. By 1920 Lutyens was admitting that his dream city would be the British "swan-song"; he could only pray it might be "a good tune, well sung." The ebullient architect of Imperial Delhi took to reading Gibbon's *Decline and Fall*. When the Prince of Wales—briefly the future Edward VIII—was first shown the model of Lutyens's Government House in 1922 he is reported to have simply snorted: "Good God!"

J. MORDAUNT CROOK
Bedford College
London, England

MILAN HAUNER. *India in Axis Strategy: Germany, Japan, and Indian Nationalists in the Second World War*. (Veröffentlichungen des Deutschen Historischen Instituts London, number 7.) Stuttgart: Klett-Cotta. 1981. Pp. 750.

For many years the old antitank obstacles in the Khyber Pass provided mute testimony to British fears during the Second World War. While the major battles occurred elsewhere, the British were most concerned that India would become the goal for Japanese-German cooperation. While never needed, the barriers reflected a certain desperation over German forays into Russia, activities in Afghanistan, and meddling in the Middle East, all coinciding with Japan's thrust from the opposite direction. The concrete blocks helped local morale.

Milan Hauner's book concerns India's role in Axis strategy. He discusses the entire period but emphasizes the earlier years of Axis initiative. His major concern is threefold: (1) the start of the war through the assault on Russia, (2) the brief period June–December 1941, and (3) the decisive year, 1942. Within that framework he provides an encyclopedic description of events with some useful details on the Germans in Afghanistan, the strange activities of Subhas Chandra Bose, and the joint Axis efforts in the Pacific. Hauner adds a useful chronology, a few documents, and an impressive bibliography.

In what is, fundamentally, a narrative history the author demonstrates that the Axis lacked any coordinated strategy, misunderstood opportunities, and could not find a base for mutual trust. In short he argues that the Axis was a union of convenience whose signatories could not overcome their selfish designs. With limited resources and fewer ideas the Axis could not pose a long-lasting threat to anyone. Even though the colonizing British were vulnerable in India, their enemies were never able to organize any assault upon that position.

The author writes well and persuasively. He deserves credit for his extensive research and for assembling a huge mass of detail. On the other hand the book's size reflects a serious problem. Hauner has included much extraneous data and marches off on numerous, unrelated tangents. He explains far more than the reader interested in the subject area really needs. Likewise his use of Japanese materials is not so skillful as his employment of German and English accounts.

In sum the book helps explain a peripheral question of the war. It collects details for a few scholars and fills a space in the literature. Like the antitank obstacles, it provides local interest and color. The main events were elsewhere.

CHARLES B. BURDICK
San Jose State University

ULF SUNDHAUSSEN. *The Road to Power: Indonesian Military Politics, 1945–1967*. New York: Oxford University Press. 1982. Pp. xii, 304. \$49.00.

Ulf Sundhaussen, senior lecturer in government at the University of Queensland, long has specialized in analyzing the army's role in the Indonesian power struggle. The present book, an expanded and reworked version of Sundhaussen's 1971 doctoral thesis, is, on the whole, also a valuable contribution to an understanding of other Indonesian political forces, such as President Sukarno and the Communist party, before their eclipse seventeen years ago.

Having had extensive access to published and unpublished Indonesian army sources, the author has been able to write a unique study of the army's own self-perception—both of its military and political mission, as well as of its leaders' views of Indonesia's constitutional democracy, the presidency, and the Sukarnoist foreign policy. Though concerned "to work out just what the Army felt" about Indonesian politics, Sundhaussen generally has managed to maintain a critical stance. Here and there, as in his analysis of the regional rebellions of the 1950s, one would have wished for and indeed expected more circumstantial detail and skepticism, especially considering the author's access to the army's own documentation. The book's title is somewhat misleading, moreover, in that it equates "military" almost exclusively with the army. In consequence, the pattern of interservice rivalries—notably the "politics" of leading air force officers like Omar Dhani in the critical period before the 1965 coup attempt—and their effect on the army are badly neglected.

Tracing the army's national history from its origins in the Japanese occupation period and the revolution against the Dutch through the constitutional crises of the 1950s and the final showdown with the Communists in the sixties, Sundhaussen convincingly shows that from the beginning a succession of top army commanders like Sudirman, Nasution, and Yani believed the army to be entitled to play a major role in government. Sundhaussen's views of the implications of the army's position are often open to some question, however.

For example, in light of the strictly nonpolitical military tradition of the Dutch colonial era, the roots of the army's claim to a political role surely must be found in the Japanese era. Sundhaussen, without critical analysis, notes that Indonesian officers themselves denied such Japanese influence. Then, too, it was not just the need to repudiate charges of Japanese "fascist" influence by political figures like Sutan Sjahrir that put the army on the defensive in the revolutionary struggle against the Dutch, as Sundhaussen argues. Rather, it was Sjahrir's strong

commitment to ministerial responsibility and parliamentary rule that similarly threatened to depoliticize the army altogether. Sjahrir's political fall saved the army's own claim.

The army's tolerance of the Indonesian parliament's aspirations to political preponderance again had worn thin by October 1954. Sundhaussen, against the evidence adduced by Herbert Feith and others, denies that Nasution by then had made plans for a coup. Yet it is difficult to interpret the army's maneuver on October 17, 1954, as other than an attempt to checkmate parliament and its struggle for public policy making supremacy.

Sundhaussen's account of the 1965 coup attempt brings little that is new and is so hedged with warnings about the deficiencies of available evidence as to cast excessive doubt on any conclusions that might be drawn. It is curious, however, that in view of Sundhaussen's access to army sources little or nothing is said in these pages about one of the most persistently puzzling aspects of the 1965 coup. That is the fact that months, indeed years, after the coup military personnel were still being arrested for allegedly having been involved in the coup "movement." The reasons why, by the army's own account, its participation in the coup was so much more extensive than was originally supposed or why the army later chose to give that impression unfortunately are not explained.

Still, despite such lacunae, Sundhaussen has written an important book that adds much to our understanding of the tangle of power conflicts in Indonesia's early national history. It is equally significant as an analysis of Indonesian events viewed through the prism of the army's leadership. As a study of Indonesian "military politics," it is perhaps less valuable but even so remains a useful starting point for further inquiry.

JUSTUS M. VAN DER KROEF
University of Bridgeport

JAMES PINCKNEY HARRISON. *The Endless War: Fifty Years of Struggle in Vietnam*. New York: Free Press. 1982. Pp. xii, 372. \$17.95.

The Endless War traces the history of the Vietnamese revolution from Ho Chi Minh's founding of the Revolutionary Youth League of Vietnam in 1925 to the fall of Saigon fifty years later. The first section of the book surveys the anticolonial revolution against France. A second section, arranged topically, analyzes in some depth the tactics and methods of the Vietnamese revolutionaries, particularly as applied in southern Vietnam in the late 1950s. A final section chronicles the major events and assesses the outcome of the Second Indochina War.

A historian of China by training, James Pinckney

Harrison sets out to explain how the Communists "took command of the Vietnamese revolution and went on to defeat enemies so many times more powerful than they" (p. ix). Not surprisingly, he finds more parallels with the Chinese revolution than either regime would now care to admit. He is also unabashedly sympathetic toward the revolution and its leaders. He concedes that circumstances and luck played a part in their ultimate success. His major conclusion, however, is that the revolution triumphed over great odds primarily because, alone among the numerous contenders for power, the Communists effectively organized the peasantry and harnessed their ideology to the intense nationalism of the Vietnamese people. "These strengths of dedication and organization," he concludes, "facilitated the incredible victories of 1954 and 1975, an accomplishment that has every right to go down as a twentieth-century version of the David and Goliath, Hannibal, or Cortez stories" (p. 316).

As a scholarly analysis of the Vietnamese revolution, the book leaves something to be desired. It is synthetic rather than original. Relying on published sources, most of them in English and French, it adds little that is new in the way of information or interpretation. The middle section, perhaps the most useful part of the book, summarizes earlier work by Jeffrey Race, William Andrews, and James Trullinger. The analysis of U.S. policy is often quite superficial. Perhaps reacting to recent defenses of the war in the United States, Harrison, on occasion, goes to the opposite extreme, indulging in overstatement more appropriate to the debate of the 1960s than to postwar scholarship. He ignores the considerable evidence of conflict between the National Liberation Front (NLF) and North Vietnam, as well as indications that from the outset Hanoi set out to dominate the southern insurgency. Noting that many of the revolution's leaders had been born in the south, he concludes simplistically that the Second Indochina War "was more of a civil war than was America's war of 1860–65" (p. 30). Responding to neoconservatives who have justified American involvement on the basis of Hanoi's recent actions, Harrison reiterates the old dove argument that the terror practiced by the NLF and North Vietnam paled into insignificance compared to the systematic destruction visited on all of Vietnam by American arms.

By focusing on the Vietnamese side and emphasizing that, in the final analysis, they won, Harrison's readable book provides a useful corrective to the currently popular notion that the United States through its own mistakes lost an eminently winnable war. Scholars and others seeking to understand the sources of the Vietnamese revolution might do better, however, to consult the more original and

balanced recent studies by William Duiker and David Marr.

GEORGE C. HERRING
University of Kentucky

KAY SAUNDERS. *Workers in Bondage: The Origins and Bases of Unfree Labour in Queensland, 1824–1916*. (University of Queensland Press Scholars' Library.) New York: University of Queensland Press. 1982. Pp. xx, 213. \$36.50.

For two generations after the European settlement of Australia in 1788, convicts numerically dominated the eastern colonies of New South Wales and Van Diemen's Land. Their punitive treatment as bonded labor—to deter potential law breakers in Britain and to develop primary industry to supply the colonizing power with raw materials—was to continue until mid-century. When the huge colonial territory of Queensland was separated from New South Wales in 1859, it still employed the last of the transported convicts.

Queenslanders, then, were accustomed to the use of bonded labor, but the colony possessed a characteristic that distinguished it from the heavily pastoral New South Wales—it had a subtropical climate. With the development of the sugar cane industry, Melanesian labor was imported from 1863 because it was docile and cheap and because it was held that Europeans could not labor in the heat of what to some pitying southern "white-neck" Australians became known as the "deep North."

Drawing many fruitful parallels with the antebellum South, the Caribbean, and Mauritius, Kay Saunders correctly stresses the essential differences in the case of Queensland's forced labor: the master had a *property* in his servants only for a fixed period of time (usually three years); the very few children were born free (the labor force being almost wholly male); and the majority of the Queensland black laborers were allowed to return home, mainly to the New Hebrides and the Solomon Islands—where too often they could find no place.

This book has well-documented sections on the development of Melanesian recruitment: from kidnapping in the 1860s and beyond to the rough-and-ready control of indentured labor by the government, which continued into this century. Then as a result of the restructuring of a sugar industry into one based on small farms and central mills, the achievement of colonial federation, and the wish of the overwhelming majority of Australians that their new nation was to be whiter than white forever, the remaining "Kanakas," as the Melanesians were known, were repatriated.

In the course of the book, the author analyzes the structure of political economy in the first plantation

colony, the origins of the "sugar kings" (often the British West Indies and sometimes Louisiana), the frequently horrifying treatment of the Melanesians (resembling that meted out sixty years later by the Japanese to Australian prisoners of war), cultural adaptations of the indentured servants, and strategies of resistance employed by them.

The book is tightly constructed and concisely written. The analysis is splendidly refracted through the use of many primary and secondary sources, including a most valuable body of Inward Correspondence once held in the Colonial Secretary's Department and now housed—uncatalogued—in the Queensland State Archives. These records clearly contain extremely useful data for historians and social scientists in fields such as race relations, poverty, and deviance.

This volume badly needs a map but is well illustrated, given the state of the original photographs and the constraints of the printing process. It is a scholarly contribution to the historiography of a tropical colony and economy characterized by ruthless cost minimization and, not least, the neurotic racism of nineteenth-century Australia.

L. L. ROBSON

University of Melbourne

STEPHEN HOLT. *Manning Clark and Australian History, 1915–1963*. New York: University of Queensland Press. 1982. Pp. xii, 207. \$17.50.

It is to be regretted that the first historiographical assessment of Australia's foremost historian, C. M. H. Clark lacks direction, focus, and coherence. In the preface to *Manning Clark and Australian History*, Stephen Holt states that "it began as a study in bibliography—a search for citations." Had Holt continued with this modest enterprise his efforts may have been better rewarded. For, by extending his parameters, he has been unable to define either to himself, or consequently to his bewildered readers, his intentions, aims, or argument.

Holt states that this is not a biography but rather "an exercise in the history of ideas." It is an uncompleted exercise that merely suggests lines of inquiry; it hints at important historiographical dilemmas and gives glimpses into the personal background and intellectual milieu in which Manning Clark operates without constructing any coherent framework in which these elements can be organized in order to produce a systematic study.

Holt feels most comfortable when dealing with Clark's personal attributes, which bear ominously and almost suffocatingly upon his creative endeavors. This latter term is used advisedly; for Clark, though indisputably Australia's foremost historian, is essentially a creative thinker and author. Clark

holds an honored place in Australia's crucial artistic triumvirate, along with novelist Patrick White and artist Sidney Nolan. All three have tried to capture the "weird melancholy" arising out of the sense of inescapable desolation and exile that has beset European civilization in this continent.

Like Patrick White, Clark is the scion of Australia's rural gentry; his maternal ancestors constituted one of the leading families in early white society. Born in 1915, Clark was reared and educated as a privileged Anglo-Australian whose primary cultural loyalty and commitment was to imperial Britain. This seemingly secure, unchallenged patrician background constituted only part of his heritage, for his father was an Anglican clergyman of English working-class origins. The class and ideological tensions emanating from such a diverse background have been crucial in molding Clark's singular vision.

This vision manifests itself most clearly in the first two volumes of *A History of Australia*, published in five volumes from 1962 to 1981, which constitutes Clark's primary achievement. He addresses himself to the fundamental contradictions involved in establishing European civilization in an ancient continent already inhabited by a people with a unique, mystical culture. Very skillfully he elucidates, through a superb if somewhat archaic narrative prose style reminiscent of Edward Gibbon but also laden with overtones of the King James Bible, a series of interrelated, continuing conflicts—those between the original owners of Australia and the invaders, between the antipodean rulers and the ruled, and between the jailers and the exiled convicts. He also gives great attention to the role of religious controversy between Protestant rectitude and Catholic piety, the impact of the Enlightenment, and, later in the nineteenth century, the almost overwhelming presence of materialism.

Essentially, the specifics of Australian history are merely the skeleton on which Clark can embody his bleak, obsessive preoccupations about the nature of good and evil in men's souls. His concern in altogether for the spiritual universe of the masculine psyche; women are relegated to the purely material, domestic domain. The influence of Dostoevsky remains overpowering, though other influences are indeed manifest. Clark has been engaged on a spiritual pilgrimage in the tradition of Bunyan and Tolstoy; like Joseph Conrad, he seeks out humanity's heart of darkness, finding in the Australian social and physical landscape its very essence. Also too can be seen the influence of D. H. Lawrence who visited and wrote about Australia in the turbulent period after the Great War when Clark was a child. Lawrence suggested the nobility of examining the only partially "civilized" offshoots of a decadent and abandoned European world. Clark's initial study of the intellectual milieu of Alexis de Tocque-

ville was to prove crucial in his transformation from a purely European-oriented intellectual to a pioneering historian of Australian society. The fusion of Tocqueville's commentaries on America, which bears so many similarities to Australia, and a study of the late nineteenth-century nationalist and novelist Joseph Furphy allowed Clark to engage specifically on Australian history. Holt's major achievement is his ability to describe and locate this dynamic process.

Unfortunately, Holt cannot come to terms with Clark's wider intellectual heritage and its impact on his thought. What is most surprising is Holt's lack of discussion of *A History*. He does allude to the controversy that raged when the first volume, *The Coming of European Civilisation*, was published in 1962 but gives no critique at all. Therefore, this book leads tortuously toward the magnum opus but, having reached it, finally abandons the undertaking altogether. It is unfortunate in other respects that Holt displays this reticence; for in volume 5, *The Earth Abideth Forever*, which covers the period 1880 to 1915, Clark's despair over the "kingdom of nothingness" reaches its nadir. Clark has taken a very public partisan stand over the 1975 coup d'état that destroyed the reformist Labour government. Volume 5 is an attempt to discover in Australian society those enduring forces of conservatism and corruption that allowed Labour's splendid vision and undertaking to be ruined. Furphy's optimistic, democratic world is destroyed and that of Dostoevsky reigns.

Nor does Holt attempt to relate Clark to other patterns in Australian historiography. Historians are mentioned either as mentors or students, but the reader has little conception of Clark's true stature or his place in the discipline. At most, all we are left with are fragments of thoughts and influences seen through a glass darkly.

KAY SAUNDERS

University of Queensland

UNITED STATES

ROBERT V. WELLS. *Revolutions in Americans' Lives: A Demographic Perspective on the History of Americans, Their Families, and Their Society*. (Contributions in Family Studies, number 6.) Westport, Conn.: Greenwood Press. 1982. Pp. xvi, 311. \$29.95.

Although demography has been around a long time, only recently, and then only sporadically, have American historians made much use of its findings. Robert V. Wells's book seeks, rather persuasively, I think, to change all that. He intends to bring demography, in the broadest sense, into the description and, more important, into the explanation of

general American history. The book is frankly a synthesis rather than a work of original research, though its grounding in the relevant literature is sufficiently deep that the publisher will soon bring out, in a second volume, a guide to the fifteen hundred works on which this book is based.

Certainly a major contribution of the book is that it summarizes a mass of information on births, deaths, marriage, family size, and migration throughout American history, from the beginnings to yesterday. The book is divided into three chronological periods, with demographic changes determining the periodization. The earliest period is from first settlement to 1770, an era Wells significantly calls "the world we have escaped"; the second, from 1770 to 1920, is when the United States achieved modernity, a characterization of those years that will not surprise readers who know Wells's previous articles. In our own era we indeed live in a different world demographically, as he dramatically illustrates by noting that in 1770 only half of the people lived to be 45; today almost everyone does. Wells is clearly on the side of modernity, which he takes for granted as a conception and does not bother to defend as an organizing principle. While he does not see the history of the American people as a simple story of improvement and gains—he is almost completely devoid of any romantic notions about Turner's frontier—he gives short shrift to those who think the past was a better time to live than the present.

But his purpose is not to praise any more than it is to denigrate the evolution of American society. Rather it is to show how changes in health, mortality, marriage, family size, and, above all, migration of diverse peoples shaped that development. He does not confine himself to facts and figures, though they abound. His aim is to suggest connections between demographic changes and other more familiar political and economic events, such as when he relates the ratification of the Thirteenth, Fourteenth, Fifteenth, and Nineteenth Amendments to changes in population, and of the Sixteenth and Eighteenth Amendments to changes in urbanization and health. His paragraphs on alteration in ethnic patterns lead him into discussion of the ethnocultural interpretation of politics, while his section on the rapid movement across the country offers a not entirely convincing explanation for the American propensity for violence in personal relations. The book also has the refreshing quality of frequently including Amerindians, blacks, and women in its generalizations about the activities of the American people.

Like many demographic historians, Wells tends to stress material changes, but he does not ignore the ways in which values are themselves sources of social change. As he writes in one place, contrary to a

commonly held view, "significant reductions in childbearing occurred long before a major improvement in life expectancy can be observed" (p. 7). The kind of impact demographic alterations can have on values is also suggested by his wondering if marriage today can have the same meaning it did a century ago now that parents live long enough to have almost as many years together *without* children as they once had with them. Is it possible that changes in mortality may be as influential in shaping society as some of the old standbys like urbanization and industrialization? One does not have to be a demographer to think, after reading this provocative book, that the answer may well be "yes."

CARL N. DEGLER
Stanford University

BRIAN W. DIPPIC. *The Vanishing American: White Attitudes and U.S. Indian Policy*. Middletown, Conn.: Wesleyan University Press. 1982. Pp. xvii, 423. \$24.95.

Brian W. Dippie has written a remarkably fine book about United States Indian policy from the beginning of the nation's history through the Indian New Deal of the 1930s. The book is not simply another survey of Indian-white relations, however, for it is built around a theme. Dippie has taken as his central thread the popular attitude toward the Indians that he calls the *Vanishing American*, a notion or concept of the Indians in America as a disappearing race. On the dust jacket, title page, and headings for the six parts into which the book is divided appears a picture of James E. Fraser's famous 1915 statue "The End of the Trail," with its mounted Indian, head bowed in dejection and despair. It is a graphic reminder of the popular concept of a dying race.

Dippie argues that Indian policies were based on the premise that the Indians were dying out and would soon disappear. The theme, however, is not as clear as the author perhaps intended, for to keep the *Vanishing American* concept alive it was necessary to shift from a belief in the physical disappearance of the Indians to a quite different idea of vanishing, that is, assimilation into white society. And beginning in the late nineteenth century the *Vanishing American* concept was met by a counter concept, as studies showed the numerical increase of persons counted as Indians. The *Vanishing American* disappears altogether in Dippie's epilogue, in which he concludes that "the native American will enter the twenty-first century unsubdued, unassimilated—and unmistakably alive" (p. 353).

The book touches all the main events and vicissitudes of Indian policy—early attempts at civilization and education, the segregation of Indians west of the Mississippi, post-Civil War reform, the Dawes

Act and allotment, the gradualism of early twentieth-century policy, the reform movement of the 1920s, and John Collier's Indian Reorganization Act—all of which Dippie treats with a sure hand and a broad knowledge of the sources. Moreover, he intended to write, he says, not so much about historical Indians as about the white perceptions of them and thus, while offering a masterful analysis of the policy variations themselves, he turns much of his attention to intellectual origins, with discussion of images in literature, scientific racist views, demographic studies, evolutionary and conservation themes, and anthropological developments. The problem, never quite satisfactorily solved, is in showing clearly how all these threads actually came to bear on the formulation of Indian policy.

The book is not organized in a strictly chronological narrative. Instead, the author has written ninety brief essays about the great variety of persons, events, policies, and programs that bear in some way on his central concern. The result is a book of somewhat loosely connected vignettes, but they are stimulating meditations for the reader to ponder and enjoy as the author moves back and forth over the broad canvas of Indian affairs. I came away enlightened and delighted, even though I wished along the way for a little tighter organization.

FRANCIS PAUL PRUCHA
Marquette University

NATHAN O. HATCH AND MARK A. NOLL, editors. *The Bible in America: Essays in Cultural History*. New York: Oxford University Press. 1982. Pp. x, 191. Cloth \$19.95, paper \$6.95.

The Bible remains the American best seller. Most citizens honor the book, four out of ten consider it to be God's errorless volume, and three out of ten claim to read it at least weekly. Yet teachers in liberal arts colleges report that even the brightest young students know little about it, many ignore it, and a few reject it as not belonging to them. Yet if Americans do not clearly possess the biblical tradition, this book shows ways in which the tradition possesses them. It appears in the names of citizens and their places, in legislation and public imagery, in the rhetoric of profound national sentiment.

A number of scholars gathered at conservative, evangelical Wheaton College in 1979 to reflect on just how the tradition does its possessing and how it came to do so. To the credit of the editors, they trimmed away fourteen of the papers presented there, many of these by prestigious historians, in order to provide some integrity of plot for this volume.

With one exception, a useful but slightly misfit piece on the Catholic search for a vernacular Bible

by Gerald P. Fogarty, the contributions follow in chronological sequence. None of the essays are duds, all of them present not always easily accessible information, a few sparkle, and most are tinged with an enlivening sense of irony about the use and misuse of the Bible.

Harry S. Stout shows how the written Word helped shape the ordered life of New England in colonial days; that New England foundation holds up under the structure of the other essays. This means that there is some neglect of the role of the Bible away from New England—for example, in the South, among large groups like the Lutherans, in the pluralism of the American West, and so forth—as the authors concentrate on those parts of the culture that gave shape to the rest of it.

Mark A. Noll reaches somewhat more widely in a piece on 1776-1865 and includes several important pages on slave religion. Nathan O. Hatch concentrates on the bonding between biblical and national symbolisms. Suddenly, halfway through the book, uncertainty and conflict develop, as reflected in the challenge of science, well chronicled by George M. Marsden, and the trauma of Fundamentalist controversy, the subject of Timothy P. Weber's chapter. Marsden and Weber have done book-length work on their subjects, and the reader gains confidence from their condensations.

Grant Wacker and Richard J. Mouw bring the story almost up to date as they chronicle, in turn, the demise of biblical civilization—perhaps a bit prematurely for American subcultures—and the kinds of impact the Bible makes today.

Weber speaks of the irony revealed in the fact that Fundamentalist approaches to the Bible have produced little fresh biblical study. Wacker sees irony in the ways biblical defenders saw their sons repudiate the defenses and the Bible. Wheaton's own Mark Noll finds irony in the fact that "those who applied the Bible's teachings to the nation's destiny most directly seemed to have understood its message least." It is a mark of maturing evangelical scholarship that these judgments can be voiced and heard. The book suggests the promise of conservative Protestant historians today.

MARTIN E. MARTY
University of Chicago

DANIEL J. CZITROM. *Media and the American Mind: From Morse to McLuhan*. Chapel Hill: University of North Carolina Press. 1982. Pp. xiv, 254. \$19.98.

It might well be that this book will not receive the attention it deserves from historians because of its title. This would be a pity, because Daniel J. Czitrom has produced a significant work that should be read by all scholars interested in the historical forces that

have shaped modern American society. In recent years many historians have begun to incorporate the study of the mass media in their overall approach to social and cultural history. Often fueled by the desire to appear fashionable, these attempts seldom go beyond the cataloguing of dates, innovations, and personalities. Motion pictures, in particular, have been adopted widely without any real insight on the part of the instructor as to how the medium can best be used to teach history. Essentially the problem lies in the propensity to focus on specific types of media content, rather than on the historically significant fact of the existence of the medium as a new form of information in society.

In this well-written volume the author has provided an overview of "the widest possible universe of historical thought and feeling about new means of communication." Thus he considers the impact of communications on American society and the response to the introduction of such technologies as the telegraph, the motion picture, and the radio. As Czitrom himself admits, he has barely scratched the surface, but the book contains a perspective on the historical role of communication technologies that should go a long way toward convincing historians to pay more particular attention to the central part played by these innovations in shaping the way we see and consider the world around us.

The book is divided into two sections: the first deals with the three forms of communication mentioned above; while the second section examines three major approaches to the study of the impact of modern media. It is the second section that provides the meatier portion for the reader wishing to incorporate media studies in an approach to history, for it lays out in a clear, concise manner the major theories that espouse the significance of communication as a historical force. Thus the author covers the work of such pioneers as Robert Park, Paul Lazarsfeld, and the Canadian media-philosopher duo of Harold Innis and Marshall McLuhan, often with greater accessibility than is found in the original work. The last two on this list have often suffered from misinterpretation and overinterpretation; Czitrom, thankfully, avoids both pitfalls, and he is therefore able to make the complex theories of these Canadian scholars available to a wider range of readers.

The book should serve notice to all social and cultural historians that they need to pay more attention to what happens to a society when a new form of communication is introduced. Through diligent research Czitrom has been able to accumulate a series of intellectual responses to such innovations that demonstrates the far-reaching influences that are often perceived. Early predictions about the influence of a medium are often quite optimistic, especially in the sometimes naive perception that

universal adoption ultimately will lead to universal brotherhood. Today we know better; but the need to study how the various forms of communication have shaped our world is a continuing intellectual problem. This excellent book should do much to stimulate historians to delve into further studies of this nature.

GARTH S. JOWETT
University of Houston

RICHARD L. RUBIN. *Press, Party, and Presidency*. New York: W. W. Norton. 1981. Pp. x, 246.

The theme of this book is pretty well summed up in the title. Richard L. Rubin is interested in how the mass media, political parties, and the presidency act on and influence each other. The metaphor of "electrical circuitry" is central to his case. If we think of politics as involving a tangled web of wires carrying messages from leadership to the people, and back from the people to leadership, it follows that changes in the circuitry affecting the velocity, range, and penetration of those messages will have profound consequences for our electoral institutions. And as Rubin points out, nothing has changed the circuitry more than the coming of television.

Historians cannot help but appreciate the author's good intentions when he argues that to understand properly what has happened in the modern era we must start by looking at periods of equivalent upheaval in the past. Unfortunately, the strategy does not quite work out in the execution. Part of the problem may be that none of the earlier upheavals seems to be remotely equivalent to what television has wrought, with the result that the two-thirds or so of the chapters devoted to history sit in disjointed relationship to the rest of the book. Just as serious, by depending almost entirely on secondary sources for his material, Rubin brings little new to the discussion other than the animating metaphor.

The historical overview would be familiar in any journalism survey course. We read about the highly partisan press of the Federalist era, the party press of the Jacksonian era, the rise of penny papers and how they in turn led to a new independent journalism, the role of newspapers and magazines in Progressive reform, and the skill Franklin Roosevelt demonstrated in shaping the channels of mass communication to his own purposes despite publisher hostility. Which is to say, we trod a well-worn path. It is perhaps carping to point out that Rubin occasionally sets up straw men, as in insisting that the so-called independent press preceded Progressivism by several decades. Few doubt it. He is also occasionally inaccurate, as in writing that Wilson held regular twice-a-week press conferences throughout his first

term and right up to American involvement in World War I. In fact, the twice-a-week conferences ended in late 1914, and by the summer of 1915 the president had abandoned the experiment of regular press conferences altogether. But these are quibbles. The important point is that the overview, at least as presented, has little or no bearing on the changes in "circuitry" that we have experienced in the electronic era.

Rubin writes with greater authority when he deals with the impact of television, although again it is difficult to say what emerges that is new. Television undoubtedly has encouraged an erosion of party structure and to that extent given the press a much more potent role than before in the presidential nominating process. The weakening ties between presidents and parties, also traceable to media influence, undoubtedly have been a factor in the recent volatility in presidential popularity. That volatility, in turn, likely has made it more difficult for presidents to take the long view in their political leadership. All of them interesting points, and all long commented on by political scientists.

The final thing to say in fairness, however, is that Rubin writes with flair. If scholars in the field are likely to find little in the book to stimulate their thought, it may very well have a place in journalism or political science courses.

GEORGE JUERGENS
Indiana University,
Bloomington

NEAL SALISBURY. *Manitou and Providence: Indians, Europeans, and the Making of New England, 1500-1643*. New York: Oxford University Press. 1982. Pp. xii, 316. \$19.95.

Since Alden Vaughan published *New England Frontier* in 1965, many scholars have been eager to revise his interpretation. He took a stab in that direction himself in a new introduction to the 1979 edition. Now Neal Salisbury offers the first part of a thorough reconsideration of the thorny problems of the Puritan confrontation with the native societies of New England. In this volume he reaches the Pequot War. A succeeding study will carry on the story. Unlike Vaughan, who was more concerned with Puritans than Indians, Salisbury writes in the ethnohistorical mode; he attempts to understand both Puritan and Indian and to explain the consequences of their meeting in America.

Salisbury argues that intellectual and social treatments of New England history have neglected the question of the Indian in the transplantation of English society. Of course he is right. Many of the major studies scarcely mention the native people.

But whether the problem of the Indian deserves the "primacy" that Salisbury seems to think it does, remains to be proved. Indeed, his own explanation tends to contradict his position. He contends that disease was the major reason for the success of the English. It was not the incompatibility of the two societies (at one point he seems to say that they were not incompatible) but rather the rapid decline of native population caused by the importation of European microbes. Shrinking Indian numbers conspired with the burgeoning colony to secure the English in America. If disease rather than cultural conflict accomplished the demise of native society, then it is no wonder that so much recent history has paid little attention to the Indians.

Of course, there is more to the story, and Salisbury devotes the bulk of his book to explaining it. The two societies, it turns out, were incompatible after all. Indian culture celebrated the reciprocity of the social, natural, and supernatural worlds. Life was shaped by continuity, abundance, and stability. In contrast, the English intruded into this idyllic setting a culture of domination and submission. Equilibrium may have been the native ideal, but by Salisbury's own account it turned out to be a shaky balance indeed. Fighting was common, certainly in the North, before the coming of the English, and strain seemed at least as characteristic as reciprocal interchange among the various bands. But most important for Anglo-Indian relations, the harmony of native culture apparently masked a high level of tension characteristic of animistic societies. The fairly rapid collapse of native culture and the creation of a new, dependent Indian exposed the inner pressures that already existed in native life. Finally, as Salisbury realizes and chooses not to stress, Puritan Congregationalism and the East Anglian village ethos offered New Englanders an equally harmonious social ideal.

Salisbury's contributions are many. He revises Cook's population estimate for New England Indians, raising the precontact figure to one hundred thirty-five thousand. His argument that the Pequots had been long established in the Connecticut Valley and that the Mohegans were a separate people at the time the English began their inroads adds a critical element to the interpretation of the Pequot War, as also does his integration of the Antinomian episode into the beginnings of that conflict. He offers a plausible explanation of the process by which Englishmen acquired Indian land, though how his version differs from Vaughan's is not clear.

Altogether, Salisbury has written a thoughtful and judicious book, a dispassionate example of what can be accomplished in ethnohistory.

BERNARD W. SHEEHAN
Indiana University,
Bloomington

DOROTHY V. JONES. *License for Empire: Colonialism by Treaty in Early America*. Chicago: University of Chicago Press. 1982. Pp. xiv, 256. \$23.00.

This is a valuable, original study, dealing with an important, previously neglected topic. It is Dorothy V. Jones's contention that the story of European advance and American Indian retreat can best be told as one of colonialism established through the trappings of international diplomacy. Her thesis is that "One of the marks of colonialism is that it bends traditional diplomatic structures to exploitative ends. This can happen because accountability is not built into the diplomatic system. The only check is the assumption of countervailing force. When that is absent, as it invariably is in situations of colonialism, the whole treaty system becomes a weapon in the arsenal of the stronger power" (p. xii). The stronger power was first the British and then the Americans. An incidental tale told is of the dominance of the Iroquois among the eastern nations, their assertions of superiority, and their failure to detect their own decline.

There are surprising bits of historical knowledge contained in the book, such as how the pre-Revolutionary controversy, notably the colonial nonimportation movements, affected Indian diplomacy. There are also revealing insights that deserve to be taken up and explored in works dealing with wider issues, such as the author's suggestion that the treaties Americans made with Indians after the Revolution should be viewed as partition treaties (p. 139), or that "Indian postwar policy was, in brief, containment of the new American nation behind the Indian-white boundary that had been negotiated with Great Britain" (p. 121). Mostly narrative, dealing with the purposes of treaties and the policies of the various parties, this book addresses topics of general interest to political, economic, and social historians. It also gives attention to some legal matters, such as the methods and concepts of land ownership among the eastern Indian nations (p. 85), showing that Indians were well aware and resentful of "the European assumption that unceded Indian land could be bartered back and forth at European conference tables" (p. 72).

Where the author occasionally stumbles is when she fails to ask questions of a technical nature, as, for example, what authority a speaker possessed when speaking for an Indian nation, or whether Indians could understand European concepts as Europeans did, such as treason, treaty, or sovereignty. Indeed, it may be asked what the author understands by sovereignty. She seems to confuse the claim to jurisdiction over land that is an aspect of sovereignty with ownership of land that is unrelated to sovereignty when she concludes that "[f]rom an assertion of full political sovereignty over the territory ac-

quired from Great Britain, the United States was forced to an acknowledgment that it had only limited sovereignty and that the limitations were set by the rights of the Indians inhabiting the land" (p. 147).

JOHN PHILLIP REID
New York University

DAVID B. QUINN, editor. *Early Maryland in a Wider World*. Detroit: Wayne State University Press. 1982. Pp. 329. \$18.50.

This fine collection of essays originated as lectures in a series at St. Mary's College of Maryland in 1977 and 1978. It is handsomely printed, unmarred by typographical errors, and carries a useful index.

The goal of editor David B. Quinn and his colleagues at St. Mary's was to provide a broad perspective on the origins, settlement, and development of early Maryland by placing these events within successive contexts to help explain why the colony took the route it did. The lectures, therefore, are grouped accordingly: "The Wider World," "The World of England," and "Forging the Chesapeake World." In the first group, J. H. Elliott and J. H. Parry each discuss imperial Spain, the first at home, and the second in the New World. Next, Melvin H. Jackson describes the techniques of seventeenth-century navigation, stressing the skills required of ships' crews. He illustrates his points by making use of an actual voyage to Virginia in 1646 of a real ship, the *Fox*. Good as it is, the essay would have benefited from the addition of a glossary explaining the many technical terms sprinkling the text.

For England in the age of exploration and colonization, G. R. Elton and Quinn each contributed pieces, Elton on English politics and society, Quinn on "Why They Came." The latter stresses the shifting forces of push and pull as settlement proceeded through successive stages. The motives and experiences of those involved early differed from those who emigrated later. Similarly, the incentives of those with means and education contrast with those who felt themselves at the mercy of a society unable to feed, clothe, or house half its population in decent sufficiency.

For an important subgroup associated with Maryland's own founding, we have a brilliant and sensitive portrait of English Catholics rendered by John Bossy. He greatly increases our understanding of their situation and why so few of them actually sought out the sanctuary George Calvert had worked so hard to prepare.

Calvert's design is the subject of a long essay on the origins and settlement of Maryland that introduces the third context. Written by Russell R. Menard and Lois Green Carr, this thoroughly re-

searched, fact-filled narrative surely stands as the definitive account of the colony's formative period. The subject, however, requires ampler scope and sustained comparison with Virginia, but that is another book.

Shifting the perspective sharply, Francis Jennings causes us to view the new arrivals in the Chesapeake from the vantage of those already there. European powers, native Americans, and Virginian predecessors all felt threatened by yet another claim to territory and trade. Building on his earlier work, Jennings pieced together from diverse sources enough clues to unfold a fascinating and terrifying story of war and diplomacy over half a continent. Using a mordant prose style and occasionally dipping his pen in vitriol, the author has scored a stunning *coup*.

Succeeding Jennings's fast-paced, almost dizzying survey of unfamiliar terrain, we next encounter an almost meditative work by Richard Dunn on the growth of slavery and its impact on white society. Adopting a comparative approach, Dunn explores the careers of a few wealthy planters in the West Indies and Virginia, taking a pair from each in three successive time periods. Despite similar origins and similar political institutions, these societies diverged sharply. Since both were built on slavery, Dunn argues that it cannot be held responsible for the development of Virginia's Revolutionary zeal for white men's freedom. He thus takes strong issue with Edmund S. Morgan's controversial thesis in *American Slavery—American Freedom: The Ordeal of Colonial Virginia* (1975). A brief review cannot do justice to the subtlety and skill with which this essay argues its point.

Closing the collection is William P. Cumming, who with enthusiasm and great charm has compiled, illustrated, and interpreted a series of maps of the Carolina coast and Chesapeake Bay from the sixteenth and seventeenth centuries. In his hands, the maps take on a life of their own as they proclaimed to potential settlers the wonders and promise of the great tidal basin and its thousands of miles of accessible shorefront.

Thus geography mixed with greed and spiced with religious idealism gave birth to a new society in the bosom of the bay. This group of essays contributes toward that broad perspective we need to understand those origins.

GLORIA L. MAIN
Colonial Williamsburg and
College of William and Mary

LOUIS W. POTTS. *Arthur Lee: A Virtuous Revolutionary*. (Southern Biography Series.) Baton Rouge: Louisiana State University Press. 1981. Pp. xiv, 315.

At one time or another during his years of public service, almost all of Arthur Lee's colleagues found him insufferable. He claimed to be more unselfishly patriotic than they were; he detected spies and traitors among pretended friends; and he lobbied aggressively against his factional opponents. Louis W. Potts's biography of Lee approaches him along several lines of inquiry, inviting the reader's sympathy while detailing Lee's personal and political foibles. We meet the revolutionary polemicist who sought a virtuous republic; the lonely, suspicious, bitter man who idealized love; and the ambitious congressman and diplomat who felt unhappy outside of government office. Each of these Arthur Lees takes his place in a different body of scholarly literature: the studies of republican ideology, the work of psychoanalytical biographers, the analyses of congressional politics and international relations. Lee would seem to be potentially as popular among current interpreters of the American Revolution as he was unpopular with revolutionaries.

Potts's book, however, is not a pastiche of secondary reading. He has studied his subject carefully; and he presents persuasive conclusions without resorting to a technical vocabulary, although he has a weakness for strained metaphors. Arthur Lee, in Potts's view, never felt assured that he existed as an autonomous personality. He remained obsessed with what other people were thinking about him. He could live only through their eyes. Constantly proclaiming his righteousness, he impugned the integrity of anyone who disagreed with him. Potts does not portray Lee as a typical revolutionary personality; nor does he make a case for a psychological causation of the Revolution or its outcome. But the book does suggest that resistance to Britain subserved personal needs that antedated the imperial crisis. And Potts concludes that Lee was "an archetype of American radicalism" (p. 69).

Most of the book is a study of Arthur Lee's activities in public office, especially his service in France and his conflicts with the other American emissaries, Benjamin Franklin and Silas Deane, as well as with their supporters in the Continental Congress. The book examines the divisions within the American mission and government, exploring the speculation, espionage, mistrust, and recriminations that exacerbated those divisions. Readers who may look askance at Potts's psychological generalizations will nevertheless find this facet of the biography useful.

Potts, like other recent biographers of American revolutionaries, has found in his subject a visionary idealist who experienced excitement in fighting evil but suffered disorientation when confronted with the moral ambiguity of business and government as practiced by his friends. As the years passed, Lee had fewer friends, could get no public office, and

ended his life as a bachelor gardener, specializing in ornamental fruit trees and roses.

CHARLES ROYSTER
Louisiana State University,
Baton Rouge

CHARLES H. LIPPY. *Seasonable Revolutionary: The Mind of Charles Chauncy*. Chicago: Nelson-Hall. 1981. Pp. xi, 179. \$18.95.

In these times, when the publication of scholarly monographs is waning due to soaring printing costs, only the best manuscripts should appear. Unfortunately, while Charles H. Lippy's biography of Charles Chauncy, minister of First Church at Boston for nearly sixty years, is well researched in primary and secondary materials and most readable, there is nothing special about this revised Princeton dissertation. To be sure, Lippy ably defends his foremost thesis that Chauncy was a conservative leader who always sought the "protection and strengthening of the New England Way . . . to preserve traditional structures, whether in religion or politics" (p. 128). Lippy carefully traces this theme through Chauncy's criticism of Jonathan Edwards in the 1740s, opposition to the placement of an Anglican episcopate in the colonies during the 1760s, support for the patriots' independence movement of the 1770s, and advocacy of Universalism in the 1780s. Moreover, Lippy convincingly argues throughout the text his secondary thesis that Chauncy "was guided by a desire to buttress what he saw as right and proper in a way 'seasonable' to time and circumstance without shattering the foundations of the established order" (p. 129; "seasonable" appears repeatedly throughout the text). But these theses are not crucial ones to early American historiography. Nor are they revelations to the well-informed, for Lippy merely documents what is known generally about the 1740-80 era with the details of Chauncy's life and thought.

Indeed, there are few historiographical surprises in this book. Lippy does amend Vernon Parrington, Perry Miller, and Alan Heimert regarding their "oversimplified liberal/conservative categorization" of Edwards and Chauncy (p. 41). Edmund S. Morgan is taken to task for denying that colonial thinkers made a distinction between internal and external taxation by the British (p. 68). Lippy also challenges Heimert's judgment that Chauncy was "too cautious in his Stamp Act repeal message" (p. 73). Finally, Lippy differs with the appraisal of Heimert and James W. Jones that Chauncy equated salvation with earning a Harvard degree (p. 118). More often, however, Lippy's work either summarizes or supports with new data the well-known interpretations of historians specializing in eighteenth-century

America. Furthermore, these historiographical quibbles and informational elaborations should have appeared in journal articles, as some did, rather than in what is essentially a "life and times" biography.

Lippy's commonplace approach is especially disappointing because he could have provided with his research on Chauncy answers to more pressing questions about early America. For example, besides rehashing and refining our knowledge of the intellectual differences between Edwards and Chauncy, Lippy could have offered new light on why the Great Awakening occurred. And Lippy seems to support without qualification Robert E. Brown's interpretation of the American Revolution by concluding that Chauncy never sought to "build a new, radically different" society but supported independence to "preserve and protect the forms and institutions of liberty which had evolved in the American context" (p. 104). Content with this conclusion about Chauncy, Lippy simply eschewed any comment on the issue of class conflict in early America. *Seasonable Revolutionary*, therefore, is the kind of informative but narrowly conceived monograph that poured off the presses in academia's affluent days before the mid-1970s, but in 1981 Nelson-Hall should have opted to publish a manuscript of greater historiographical significance.

GEORGE SELEMENT

Southwest Missouri State University

MARY BETH NORTON. *Liberty's Daughters: The Revolutionary Experience of American Women, 1750-1800*. Boston: Little, Brown. 1980. Pp. xvi, 384.

Whether or not the Revolution significantly altered the lives and perceptions of American women has become a hotly contested issue. Mary Beth Norton contends unequivocally that it did, but that its impact cannot be deciphered from examining legal codes and political participation. Instead, she affirms, that impact "is more accurately revealed in an analysis of women's private writings" (p. xv). After scrutinizing 368 unpublished collections of family papers, Norton finds that white women by the 1780s were "reading widely in political literature, publishing their own sentiments, engaging in heated debates over public policy, and avidly supporting the war effort in a variety of ways" (p. 156). Constant talk of politics at home, participation in patriotic spinning bees, home substitutions for goods previously acquired from Britain, and the experience of "controlling their own affairs" during wartime caused women to abandon their "natural" feminine timidity, to achieve a greater "reverence of self," and to believe in their own rational capabilities. They also postponed or gave up marriage, practiced

contraception, petitioned for divorce, and attended school. Men in the young republic began to value intelligent women, praised them for their efforts, and expanded educational opportunities for them. Norton is aware that not all women and not all things changed—after the Revolution, as before, women were expected to find "happiness in their chimney corners," to assume the central roles of wife and mother, and to behave in a modest, delicate, pious manner.

Norton argues a complex and difficult-to-prove position with enthusiasm, a clear style, a fine selection of spritely illustrative quotations (nieces are called "such sweet toads," women "trifling insignificant Animals"), and enough evidence from widely selected sources to demonstrate that changes were occurring in private, if not public, life.

Norton's conclusions are compelling but not thoroughly convincing. Her assertions are too inclusive at times. She provides little information to demonstrate that a decline in family size among Hingham, Massachusetts, and middle colony Quaker families represents a nationwide effort to limit births, or that an increase of divorce petitions by Connecticut and Massachusetts women suggests a wider trend. Nor do the records of a handful of plantations provide us with much information about the experiences faced by black slaves across the nation. Although Norton must be applauded for her effort to include female slaves in nearly every chapter, her total avoidance of Northern slave women severely limits her generalizations. Moreover, she does not pay attention to the tremendous diversity in America—to the fact that Scottish, Irish, German, French, Jewish, and Indian women of many tribes comprised substantial numbers in Revolutionary America. Creek Indian women are mentioned briefly in two places; the other groups' separate ways of perceiving sex roles and women's progress are totally unexamined.

When black women are considered, there is little effort to place them in the context of Afro-American culture. Did these women reproduce African patterns of marriage and community life in the New World? Were these patterns given greater vitality by the Revolution? What about conjure women and female preachers? Norton tells us nothing about such concerns. In fact, her view of the Southern plantation owner is essentially benign. Cruelty to black women is barely touched upon, and black women's discontent as revealed in arson attempts and sometimes murders is ignored. Their lives as runaways in backwoods maroon communities are not mentioned. Norton states that more than sixteen hundred black women ran away to the British during the Revolution, but the reader is left wondering why. Moreover, she gives only a few lines to the rape of black women—a common eventuality

over the centuries—but supplies more than two pages of detail on the rape of white women by the British—a rather less common event.

Norton does draw valid conclusions for the way the Revolution affected many female members of the “better and middling sorts” (even though, at times, it is difficult to determine whether a given woman’s response is idiosyncratic or normative). She extends these to the “meaner sort” as well, however, stating that “it seems possible to allow the literate portion of the female population to speak for their illiterate counterparts” (p. xvi). This is an extremely dubious assumption, even when the concerns at issue are the “universals of female lives—courtship, marriage, pregnancy and childbirth, child rearing, and household works.” Do Iroquois women and black slaves share similar attitudes toward courtship, marriage, and so forth? Do Pennsylvania-Dutch *hausfrauen*? Norton must prove that they do. She does not.

It is not that Norton is unaware of sex role differences—she points out that rural women labored at more “male” tasks than their “better” urban counterparts, were more active as girls in the daily round of production, and kept their houses less clean. But she fails to view such differences as crucial. Her thrust is to homogenize the experiences of eighteenth-century women beyond what would appear reasonable. Too often, the rich have spoken for the poor, and Norton does nothing to correct this tendency. Had Norton restricted her conclusions to the “better and middling sort,” those women who authored her sources, *Liberty’s Daughters* would have been an excellent, well-reasoned work. It is still useful, and had she relied on those sources that depict the attitudes of the poor and ethnic groups—the court records, newspaper accounts, more travelers’ observations, and the records of Indian missionaries—she could have provided evidence of how the Revolution affected “the lives of all eighteenth century American women.”

LYLE KOEHLER
University of Cincinnati

LAWRENCE DELBERT CRESS. *Citizens in Arms: The Army and the Militia in American Society to the War of 1812*. (Studies on Armed Forces and Society.) Chapel Hill: University of North Carolina Press. 1982. Pp. xvi, 238. \$22.50.

Every government has the right to defend itself. But a standing army is too often the instrument through which tyrants have come to power and destroyed the people’s liberties. In *Citizens in Arms* Lawrence Delbert Cress traces the ideological debate over this dilemma from the American Revolution to the War of 1812.

Seventeenth-century radical Whig philosophy dictated that the principal reliance for defense lay with the citizen-soldier. A militia of virtuous citizens would recognize their duty to defend the government and as property holders they could never be subverted by tyrants. American Whigs in the 1770s adopted this view in protesting the presence of redcoats in the colonies. Unfortunately for these theorists the War of Independence demonstrated that when called to arms most “virtuous citizens” stayed at home. Men could not even be induced to serve short-term militia enlistments without the promise of bounties and pay. Self-interest obviously outweighed virtue.

Radical Whig philosophy was also altered by a more moderate view. In the modern world war was too complex for the average citizen called suddenly into service. But was there not a place for the professional soldier who could take his place in modern society alongside the merchant and the artisan? A small standing army to meet threats of insurrection and to provide a cadre of professionals might be acceptable.

Cress points out that the new republic had also to grapple with another dimension of the debate, that is, the question of whether the army was to be controlled by the central government or the states. The national government was viewed by many as the “tyrant” whose grasp for military power would subvert liberty.

At the end of the war the army virtually disappeared, but the impotence of the Confederation government in the face of the Indian menace, Shays’s Rebellion, and British occupation of the Northwest posts posed the hard question: How was the nation to preserve liberty and also defend itself from internal and external threats?

The new Constitution did little to solve the problem. It authorized Congress to raise an army and to “call forth the militia” but it left the question of control to Congress. Not until the twin menaces of Indian attacks and the outbreak of war in Europe did Congress take any decisive steps. By 1796 the necessity for regular troops to garrison the frontier forts was accepted by Congress. The Quasi-War with France pushed Congress into providing for a truly national army of twelve thousand men completely independent of state control. But as Cress notes, the Federalists overplayed their hand. It became apparent that some High Federalists wished to use the army to suppress Republican opposition. Perhaps equally important was the fact that Federalist policy had produced high taxes—and no war. As the hysteria of 1798–99 subsided and the Jeffersonian “revolution” brought Republicans to power, it was natural that radical Whig ideology should have a resurgence. The army reverted to its role as a garrison for the frontier and even moderate militia

reforms advocated by Jefferson and Madison found little support in Congress. Thus the nation entered the War of 1812 as little prepared as it had been two decades before.

I am not sure I agree with one of Cress's conclusions that in the War of 1812 "only the cessation of hostilities . . . stopped the further degeneration of the army and the possible destruction of the American republic" (p. 173). The army's poor performance was due as much to poor generals and bad administration as a lack of quality and quantity in its manpower. And far from degenerating, the army—and the navy—defeated the British campaign of 1814 at Baltimore, Plattsburg, and New Orleans. And I am not sure that Calhoun's plan (rejected by Congress) was quite the milestone that Cress makes it. Rather it would appear that the general ideological acceptance of a regular peacetime establishment was the outgrowth of the Federalist efforts of the 1790s.

But these are minor flaws. Cress has very ably presented an important debate on the ideological conflict over military policy that is usually overlooked by historians, but which had profound implications for almost all the major issues of the early national period.

JOHN PANCAKE
University of Alabama

REGINALD C. STUART. *War and American Thought: From the Revolution to the Monroe Doctrine*. Kent, Ohio: Kent State University Press. 1982. Pp. xvi, 245. \$19.50.

Reginald C. Stuart's study is an intellectual history of American attitudes toward war from the Revolution to the Monroe Doctrine, and purports to show that the Revolutionary generation believed in a limited-war mentality that sought to control rather than to eliminate armed conflict. This mentality was derived in part from numerous antecedents in Europe—which indicates that the myth of American uniqueness in waging war has obscured how much this country has been a part of the Western world in military matters—as well as from America's colonial experience. When the Revolutionary leaders faced the needs of the new United States, however, they believed in using war, wherever necessary, as a policy of state. Two forces—American republican ideology (whose orthodox belief was that monarchies made war while republics stood for peace) and strident nationalism—transformed the limited-war mentality into an American myth of war.

The American myth of war presented a dualism. On the one hand, it could be aggressive and self-righteous because of the powerful nationalism that propelled it. On the other, it could be restrained and

reluctant to resort to force. James Monroe, according to Stuart, was the transitional figure in this evolution of American attitudes to war that carried over the legacy of limited-war mentality to the philosophy of war and peace that affected later generations.

In his concluding chapter, Stuart suggests in a brief survey that the attitudes of the Revolutionary generation may have left a legacy of limited-war mentality whose echoes could be heard down through the nineteenth century and even into the modern period with the advent of the Cold War and nuclear weapons. This legacy, he contends, is contrary to the legend held by many scholars of the American view of war. The legend holds that Americans rejected the Clausewitzian definition of war as the continuation of politics by other means, and argues also that the dominant ideology in America was liberalism, which assumed men were rational and consequently should be able to resolve their differences with peaceful solutions.

This is an ambitious book, but one that presents some serious problems. The first is inherent in the nature of intellectual history: the slippery business of tracing the influence of ideas, which Stuart does not always do successfully. The second is the matter of whose ideas should be examined on the subject. Stuart confines himself mainly to politicians and political leaders and to scholars regarding the legend of the American view of war. This is far too narrow a segment of society to support the case he hopes to make, especially in view of the political role the broad mass of society played in American wars. Despite these problems, Stuart has written a thoughtful book that should help to correct the easy generalizations historians have made on American attitudes toward war that often disregard the beliefs of the Revolutionary generation leaders. This reviewer agrees with Stuart's statement that "this study represents a beginning, rather than a final word, on the subject considered."

GEORGE A. BILLIAS
Clark University

MARY P. RYAN. *Cradle of the Middle Class: The Family in Oneida County, New York, 1790–1865*. (Interdisciplinary Perspectives on Modern History.) New York: Cambridge University Press. 1981. Pp. xiv, 321. \$24.95.

Some years ago J. H. Hexter remarked that historians have a curious love affair with the middle class: behind every great event they invariably detect the relentless influence of the bourgeoisie, rising ever upward on the curve of history. Yet, in the context of the United States, no one has ever successfully defined the middle class as a class, perhaps because

it is an article of middle-class faith that America has no classes at all. Increasingly, though, the pursuit of the elusive bourgeoisie has intensified, especially on the part of historians of mid-nineteenth-century America who, in seeking to explain the character of a new democratic, capitalist culture, have fastened on the middle-class ideology of free labor and upward mobility and the creation of a new ideal of educated professionalism.

Now, Mary P. Ryan adds significantly to the developing picture. In her interpretation the key to the middle class can be found in a new pattern of family life, with father at the office, a loving mother safe in her proper "sphere" at home, the children under her care or at school, and the parents jointly committed to rearing the young in those habits of industry and self-restraint calculated to ensure successful marriages and careers. "Early in the nineteenth century," Ryan contends, "the American middle class molded its distinctive identity around domestic values and family practices" (p. 15). The making of the middle class began and ended at home.

Cradle of the Middle Class sets forth this interpretation through an intensive community study of Oneida County, New York, and the city of Utica, in particular, from frontier settlement through an expansive era of "small-scale commercial capitalism" to the eventual coming of industrialization. As a work of social description, it is thickly packed with quantitative details about demography, economy, household composition, and the life course, all assiduously culled from censuses, city directories, probate records, and the like. Ryan also draws on diaries, letters, novels, sermons, and the files of Oneida's many church and voluntary societies to explore the ways people experienced the deep social changes in their lives. Unfortunately, all of this painstaking research does not pay off. Ryan's book is badly written and carelessly reasoned, so caught up in the domestic drama of the middle class that it never reflects on its basic premises at all.

Part of the trouble is the overblown, hectic language. Ryan personifies abstractions, investing them with great gusto ("the spirit of cash and capital had run riot over Whitestown's farms and along Utica's streets" [p. 9]), all the while draining the real people of Oneida County of their once vigorous life. This reifying rhetoric undercuts the genuine possibilities of the book. Ryan carefully traces the rise and fall of the patriarchal, corporate family in early agrarian Oneida (though without any awareness of the important debate on this subject prompted by James Henretta), then turns to the resulting crisis in family and society and the quest for new ways to organize relations between the sexes and between the generations. That approach is well taken, but Ryan's substantive conclusions are unconvincing.

Consider the treatment of religious revivals, a

phenomenon of major significance in Oneida, which was an integral part of New York's colorful "Burned-Over District." In Ryan's view, evangelical religion and the social reforms it spawned were central to the redefinition of family life along middle-class lines. Indeed, the very success of the revivals, she asserts, derived directly from the persuasive influence of mothers, who piously led their children into the church. But in fact, children of the church formed only a small minority (30 percent) of the saved; even they could have converted defiantly in spite of their parents, as some contemporaries charged, and not dutifully because of them. In her eagerness to show the "feminization" of religion, Ryan never pauses to analyze these possibilities. She consistently chooses to infer the meaning of movements from the social characteristics of participants, rather than to study their rhetoric and activities in depth.

Most disturbing of all, Ryan has no satisfying explanation for why the middle class developed the ideology of domesticity and the family forms that it did. The patriarchal household, she does make clear, collapsed from economic causes—declining farmland and the advance of the market. But why was it succeeded by the Victorian home? Oddly, Ryan credits an "old middle class" of artisans and shopkeepers, still bound up with the corporate household, with generating the new ideal of domesticity. This "old middle class" is a curious category, which ignores the distinction between journeymen and master craftsmen in an age of increasing inequality. The group represented embattled traditionalists, anxious about social chaos in the expanding city. But it was not alone in fostering domesticity. Women of the elite, the first to experience the new separation between work and the home, joined the cause. And by the 1850s, Ryan says, the torch of domesticity was passed on to a "new middle class" of salaried clerks who put the rhetoric into practice in a newly privatized home. Every complex social movement, of course, has many contributors. But calling all these actors "middle class" and enlisting them in a common campaign serves only to rename the problem, *not* to solve it. Explaining the transit from the patriarchal household to the Victorian home demands more than a resort to that old historical standby: the rising middle class.

ROBERT A. GROSS
University of Sussex

BERTRAM WYATT-BROWN. *Southern Honor: Ethics and Behavior in the Old South*. New York: Oxford University Press. 1982. Pp. xxiv, 597. \$29.95.

During the Renaissance, the word *honor* underwent a transformation in meaning, away from its medi-

eval association with high social position and toward its modern identification as a mental attitude. Thus, Samuel Johnson defined honor as "nobleness of mind, scorn of meanness, magnanimity." Bertram Wyatt-Brown rejects this newer meaning as misleading. True honor, he argues, remained a behavioral pattern enforced by the desire to retain class rank. What Doctor Johnson defined, Wyatt-Brown calls "gentility," and he relegates it to a minor role in *Southern Honor*. Rather, "the determination of men to have power, prestige and self-esteem and to immortalize these acquisitions through their progeny was the key to the South's development" (p. 16). In the North, personal conduct during the nineteenth century was increasingly guided by guilt, an internal sense of right and wrong. But in the Old South, the modern, inner-directed personality made few inroads. There, the traditional, other-directed personality predominated; conduct was guided by shame, by the society's opinion of one's behavior. Implicitly, then, Wyatt-Brown stands David Riesman on his head. Moreover, he asserts, "It was this discrepancy between one section devoted to conscience and to secular economic concerns and the other to honor and to persistent community sanctions that eventually compelled the slaveholding states to withdraw" from the Union (p. 20).

These are bold claims. Unfortunately, a reading of this volume leaves one convinced that Wyatt-Brown's portrait of the South, far from inhering in his evidence, is virtually imposed on it by main force. In the first place, it is Wyatt-Brown's position that "the difference between Northerner and Southerner was often slight, although collectively the societies were changing at different rates" (p. 515 n.). Therefore, Wyatt-Brown refuses to be troubled by the fact that much of the conduct that he takes to define Southern society is to be found throughout the nation. And the fact that the nineteenth-century South offers many exceptions to his generalizations is also unimportant; indeed, he concedes at the outset that the antebellum South—whose very essence, he says, is to be found in hierarchy and devotion to honor—was neither so rigid nor so honor-bound as are societies of the Mediterranean basin today (p. xiii).

But his dismissal of contrary evidence is not the worst of it. So convinced is Wyatt-Brown of his thesis that when evidence is lacking, he sometimes simply assumes it. It is true, he notes, that many Southern families' letters reflect childrearing on the Northern guilt-centered model. "One must suspect, however, that there were more traditional parents—ones who were not careful about keeping home records—than there were those who did so" (p. 146). He concludes, "in the light of the general level of violence" in the South, that drunken assaults by husbands on wives "probably exceeded the numbers elsewhere in the country, and extended through a wider range of the

social classes" (p. 281). He notes that Northerners aided kinsmen "with no less and no more willingness than Southerners. However, there was, one may speculate, a sense of deeper obligation in the South" (p. 332). We are even told that, because of "cultural ideals" in the South, "a sense of paralysis gripped" Southern matrons faced with the challenge of keeping house (pp. 334–35). And when Southern men gambled at cards, "the point of play was the distribution of honor and status" (p. 340).

These speculations are the most extreme examples of the deductive style of argument that characterizes the book. At every stage Wyatt-Brown assumes what he seeks to prove. He has placed his case for a unique antebellum Southern culture on the relative regional rates of decline of the ideals that he describes. One might therefore have expected that he would have given special attention to chronology and to careful interregional comparison. In fact, however, the evidence is as likely to date from the eighteenth century or the twentieth as from the early nineteenth, and Northern analogues seldom appear at all. The behavioral patterns to which Wyatt-Brown alludes, when they exist, could as well divide antebellum rural from urban society as North from South. At the very least, one may conclude that the notion that they define the essential antebellum South remains far from established.

J. MILLS THORNTON III
University of Michigan,
Ann Arbor

MARK E. NEELY, JR. *The Abraham Lincoln Encyclopedia*. New York: McGraw-Hill. 1982. Pp. xii, 356. \$45.00.

There have been so many books, articles, and pamphlets written about Abraham Lincoln that it becomes increasingly difficult for any one person to master all facets of this vast literature. These works run the gamut from accounts of rather standard events in Lincoln's life and career to more exotic speculation about whether our sixteenth president suffered from a disease called Marfan syndrome. Mark E. Neely, Jr., attempts to bring some order out of this chaos with his *Abraham Lincoln Encyclopedia*.

One of the major problems associated with writing an encyclopedia, particularly one dealing with a single topic, is determining what subjects should be included and how to present them in a coherent manner. As director of the Louis A. Warren Center for Lincoln Studies in Fort Wayne, Indiana, Neely is admirably suited for his task. Drawing on his own experience with the questions asked about Lincoln by both amateur and professional historians and his knowledge of Lincoln historiography, he has produced a book that is both well focused and comprehensive.

Neely purposely avoids making his work just

another Civil War encyclopedia. There are no battle entries on the grounds that Lincoln was not a field commander. Among the entries he does include are biographical sketches of Lincoln's contemporaries and associates, a discussion of great Lincoln collectors and historians (although only those who are deceased) as well as modern-day Lincoln groups and associations. His unified approach is seen in the biographical treatments where he attempts to answer the same series of questions about each individual.

The encyclopedia also deals with materials in areas often neglected by academic historians, such as Lincoln's assassination. While this emphasis on the assassination may not suit all tastes, his judicious treatment of this topic provides a welcome corrective to the sensational theories that abound concerning that event.

Neely's own research on Lincoln often adds fresh insights as well. His investigation of the Illinois voting rolls has revealed that Lincoln's relatives were not all Democrats as historians have claimed. Lincoln's father was a Whig and his mother's cousin, Dennis Hanks ran on the Whig ticket for a minor office. Conversely, the Clary Grove boys who are usually portrayed as staunch Lincoln supporters actually voted for his Democratic rival, Peter Cartwright, in 1846.

Other features that greatly enhance the encyclopedia are the numerous prints, cartoons, and photographs as well as an excellent index. Each article also contains a bibliography that draws heavily on primary sources, such as the *Collected Works of Abraham Lincoln* and the Abraham Lincoln papers in the Library of Congress, as well as the major secondary works. This provides the reader with the most up-to-date scholarship on the subject.

In short, Mark Neely has produced a major reference work, of value both to those who are intimately familiar with Lincoln and those who are just beginning that acquaintance. He has also demonstrated that an encyclopedia need not be as dry as dust. The book is written in a lively style, comprehensively covers its subject, and makes a major contribution to its field. Neely deserves the plaudits of Lincoln scholars everywhere.

THOMAS R. TURNER
Bridgewater State College

PHYLLIS F. FIELD. *The Politics of Race in New York: The Struggle for Black Suffrage in the Civil War Era*. Ithaca: Cornell University Press. 1982. Pp. 264. \$19.50.

In 1821, New Yorkers altered their state's suffrage requirements by abolishing property qualifications for whites and increasing them for blacks. On three occasions over the next four decades, champions of equal manhood suffrage unsuccessfully attempted

to remove this racial distinction through popular referenda. Voter opposition to equal suffrage declined over the years, from 72.4 percent in 1846 to 53.1 percent in 1869. Nevertheless, black suffrage came to New York—as it came to fully half of the Northern states—not by state action but by the ratification in 1870 of the Fifteenth Amendment to the United States Constitution.

In this excellent book, Phyllis F. Field brings to the study of politics and race the quantitative sophistication of the "new" political history. In general, Field's data fit nicely within the analytic categories first outlined by Lee Benson in *The Concept of Jacksonian Democracy* (1961). For Benson, and for those who have followed his lead, "ethnocultural" distinctions shaped mass political behavior in New York and across the antebellum North. Thus, native-born Protestants of New England origin identified strongly with the Whig and Republican parties. Foreign-born Catholics, predominantly Irish and German, gave their support overwhelmingly to the Democrats. Beyond these distinctions, political parties functioned as nonideological, opportunistic, vote-gathering coalitions with little discernible attachment to consistent theories or principles.

Historians concerned with the relationship between political ideology and emerging class distinctions will find Field's analysis of the politics of race rewarding. Pointing to the sharply polarized voter response to Negro suffrage proposals, Field contends that parties used the race issue to reinforce ethnocultural voting behavior. Both parties manipulated "the social fears of their constituent elements" and used these fears "as a kind of cement to bind the party together at the grassroots level" (p. 230). Thus, "Democrats used the race issue to strengthen their hold on foreign born, working class, anti-evangelical voters" (p. 61). Whigs, and later Republicans, supported Negro suffrage to consolidate party loyalty "in areas where Yankee culture was a relatively fresh import" (p. 70).

Support for Negro suffrage came most concertedly from the "burned-over" district, from emerging manufacturing towns like Rochester where, as Paul Johnson has shown, the moral reform enthusiasms of the Second Great Awakening expressed an emerging middle-class drive for autonomy and self-control. Field discovers that Yankee voters who supported temperance reform heavily supported Negro suffrage as well. Conversely, the lowest levels of support came from immigrant working men's districts in New York City where evangelical moral reform (with temperance in the lead) threatened to impose Yankee cultural hegemony. By the late 1850s, Republicans had "worked out an interpretation of the [Negro suffrage] issue that linked it to . . . the dual threat posed by the southern slaveholder and the Catholic immigrant" (p. 111).

In the postwar years, with the Democrats identi-

fied more closely than ever with immigrants and Negrophobia, Republicans continued to appeal "to those who wished to see a greater homogeneity of values achieved *through laws*" (p. 230). But, the pursuit of racial equality no longer concerned them. Instead, Republicans insisted that the Fifteenth Amendment resolved the long-lived and troublesome "Negro Question." The image of the down-trodden black man, having served its purpose, passed from the political scene.

LOUIS S. GERTEIS
University of Missouri,
St. Louis

E. B. LONG. *The Saints and the Union: Utah Territory during the Civil War*. Urbana: University of Illinois Press. 1981. Pp. xiii, 310. \$17.95.

The Saints and the Union is a detailed narrative of the difficult relations between Mormon-controlled Utah and federal authorities during the Civil War. E. B. Long, until his death in 1981 professor of American studies at the University of Wyoming, has contributed what will undoubtedly become the standard account of this "sideshow" in the American West. He has told the story of this clash between Mormon and federal authority with skill, attention to detail, and good humor.

Mormon relations with Washington had never been friendly, and with the outbreak of the Civil War the Saints proclaimed the death of the Union and anticipated the organization of a Mormon theocracy. Consequently, U.S. officials considered the Saints less than fully loyal and took action to ensure Utah's continuation in the Union by sending an occupation army to the territory. General Patrick E. Connor and his California Volunteers entered Utah in 1862, promptly raising Mormon hackles with heavy-handed tactics. Connor's efforts were opposed by Brigham Young, Mormon prophet and pragmatic politician par excellence, who desperately sought to maintain church hegemony. Throughout the remainder of the war each battled the other with words and complicated political maneuvering.

Long's narrative describes this war between Young and Connor with an almost day-by-day attention to detail, but this exacting recitation is both the strongest and weakest aspect of the book. Long was an outstanding researcher—his work as research assistant to Bruce Catton clearly established his skill—and it shows in *The Saints and the Union*. He mined sources nationwide, including those in the now-closed Mormon church archives in Salt Lake City, and quoted them freely in the text. Yet at times the lengthy quotations and the account of the day-by-day struggle between Young and Connor hinder understanding. Certainly, this dialogue blocks the more reflective analysis that Long could

have written. One wishes Long had placed his story more in context with the questions of Western state making and local versus national political concerns. Thomas G. Alexander's *A Clash of Interests* (1977) and Howard R. Lamar's *The Far Southwest* (1966) laid ample groundwork for considering these questions in Utah during the Civil War. Conversely, Klaus J. Hansen, *Quest for Empire* (1967), delved deeply into the meaning of the Mormon struggle for autonomy. Long could have profited by using those studies more fully. Still, *The Saints and the Union* is a sound historical work and will plug a gap in Mormon historiography. It tells the exciting tale of Utah during the Civil War and ultimately fails only when one tries to find meanings that go far beyond the mundane account.

ROGER D. LAUNIOUS
Military Airlift Command
United States Air Force

WILLIAM W. SAVAGE, JR. *The Cowboy Hero: His Image in American History and Culture*. Norman: University of Oklahoma Press. 1979. Pp. xii, 179. \$13.95.

Here is yet another book on that deathless American hero—the cowboy. William Savage has written ten chapters, some of them thinner than a cowboy's hips, in which he attempts "an excursion into the American mind selecting a reliable vehicle with considerable mileage on it" (p. xi).

The result is interesting, but uneven. The opening chapter, "History, Culture, and the Cowboy Image," is promising. Here Savage suggests the limitations of research on the cowboy and usefully punctures the assertion by Philip Durham and Everett Jones in *The Negro Cowboy* (1965) that there were five thousand blacks that trailed north from Texas after the Civil War. He also comments on the difficulty in getting at what motivated men, especially youngsters, to take to "cowboying." One subjective but very persuasive explanation that Savage might have cited shows up in *Cowboy* (1928) in which Ross Santee describes unforgettably the potent pull of punching cattle on boys longing for release from the dullness and sense of stagnation on family farms.

The next chapter, "Cowboy Thought in America," belies the earlier promise. Cowboy thought turns out to be a composite of common sense, racist attitudes, and irrelevant witticisms. If that comment seems inconclusive and vague, so does the chapter. There follow brief examinations of ways of playing cowboy from dressing up to counting down; variations on Western music—folk, country, rock; cowhands and women; marketing the sagebrush hero; and riding the rodeo circuit. In the next to last chapter, one of Savage's most original, he examines the "long drive" in fiction and film as a matter of growing up, and then turns to a contrasting

theme—the juvenilization of the cowboy (Savage calls it “growing down”), as in the purification of Clarence Mulford’s rangeland rowdy, Hopalong Cassidy, through the television series featuring William Boyd. In summing up, Savage points out the cyclical pattern of cowboy popularity in public taste finding him “but one of many wellsprings of national experience” (p. 163).

Savage neglects one medium that exploits the cowboy image: the comics. Where are “Young Buffalo Bill,” “Red Ryder,” “Cowboy Bob,” and most of all “Rick O’Shay?” Never mind. Through various angles of vision, Savage helps the reader better understand the popular image of our “ten-gallon hero.” For that, the many members of the cult of the cowboy can be grateful.

EDWIN R. BINGHAM
University of Oregon

RONALD H. LIMBAUGH. *Rocky Mountain Carpetbaggers: Idaho’s Territorial Governors, 1863–1890*. Moscow: University Press of Idaho. 1982. Pp. 234. \$19.95.

Residents of Western territories long railed against appointed officials sent out by Washington and complained that the territories were a kind of Botany Bay for the dumping of needy relatives and political vagabonds. Nineteenth-century Idahoans were no exception, and in this book Ronald H. Limbaugh details both their criticism and the realities of political life in that remote area. Throughout, Limbaugh makes the point that, despite constant rhetoric damning carpetbag appointees, the real complaint was not so much a lack of home rule but who should rule at home: the basic theme remained “local spoils for local politicians” (p. 38). Behind the condemnation of “pilgrims” from outside were hidden less altruistic questions of patronage, local power politics, factional intrigue, party infighting, and personal or sectional rivalries. Territorial home rule, specifically Idaho home rule, was not democracy sprung directly from the frontier, à la Turner.

The book focuses on Idaho’s territorial governors. They were a motley bunch, these “hothouse politicians”—capitalists, newspaper editors, Civil War generals, a former congressman, and Rutherford B. Hayes’s former private secretary, among them. Of sixteen appointed, three never reached Idaho, three served only a few months each, and only the last two, 1885–90, were residents when nominated. Once on the scene (and absenteeism was great) they were quickly pulled into the bare-knuckled political brawls that dominated the local scene. In Idaho, as elsewhere, tensions mounted high over the Civil War and Reconstruction and died hard. Personalities, party strife, and especially sectional issues split the territory almost from the beginning.

Unhappy with the location of the capital in Boise, northern Idaho fought to move it and, that failing, sought annexation to Washington. The southwest, with Boise as its hub, was dominated by a Republican clique, first labeled the Oregon ring, then the Boise ring. And the southeast, growing in numbers with expanding railroads and mining strikes of the early 1880s, constituted a third major factor. Moreover, the southeast included a substantial Mormon population, which provided the most explosive issue and set the stage for a bitter anti-Mormon movement that culminated in 1884–85 in oppressive legislation that in effect isolated and disfranchised the Saints. By this time, party affiliation had been largely replaced by shifting coalitions of interest groups.

This is a tight, well-written book based on extensive sources, manuscript and printed, official and private, though with a neglect of published material since 1974. Sixteen full-page photographs enhance the volume, but an appendix listing members of each legislative session has dubious value. If the themes are not totally new, the Idaho story makes an excellent case study. Set a little more fully into context, perhaps with stronger comparison with sister territories, it would be even better.

CLARK C. SPENCE
*University of Illinois,
Urbana-Champaign*

THOMAS J. NOEL. *The City and The Saloon: Denver, 1858–1916*. Lincoln: University of Nebraska Press. 1982. Pp. xvi, 148. \$16.50.

Thomas J. Noel traces the origin of his book to an inspired evening with friends in 1972 that produced the idea of a historical survey of every saloon in Denver. Under the spell of these memories he forgets to mention his story in the *Empire Magazine* of the *Denver Post* in 1973 and his article, “The Multifunctional Frontier Saloon: Denver, 1858–1876” (*Colorado Magazine* 52 [1975]: 114–36). The latter piece has contributed much to the beginning of this slim volume, including an identical opening based on Lavinia Porter’s recollections of reaching the South Platte and Denver in 1860, with an adjustment for the book so that she and her husband view the Rocky Mountains and not Longs Peak as in the article.

The change is indicative of other features of the work, which extends the story to New Year’s Day, 1916, when the saloon vanished as Colorado went dry. There are eight maps that give the locations of taverns at intervals between 1860 and 1915, the downtown saloons in 1900 and 1915, the city’s streetcar suburbs, and the ward boundaries at the beginning of the twentieth century. These plans

strengthen the cohesion of a discussion of the various influences that affected the saloon. The topics range from individual thirsts to public attitudes, and from residential demography to big breweries stripping saloonkeepers of their independence. Three picture sections illustrate the reputable and seedy aspects of the setting; the central one is devoted to Denver's man-behind-the-scene, saloonkeeper Edward Chase.

Less colorful men form the human focus of the investigation. With the records of their careers between 1858 and 1885, primarily taken from city directories, the author probes saloonkeeping as an avenue of social mobility. Here a set of tables relates birthplaces, changes in job status, number of positions occupied, and upward and downward movements of saloon-men. In light of the great turnover, the author suggests that in Denver selling liquor furnished an often-explored entrance into the class of small businessmen. A shift in the composition from native-born to immigrant saloonkeepers, which he dates at around 1870, allows him to discuss the significance of the taverns for Irish, German, Jewish, Italian, and Slavic immigrants and its connection with ward politics and boss rule. He also identifies the resulting estrangement of other Denverites from the saloon, ultimately serving distinct neighborhoods in declining residential areas surrounded by expanding suburbs, as one reason that made the tavern vulnerable to reform.

Among the interesting observations are a few startling asides. A reference to "the rosy city history completed by Jerome Smiley in 1901" (p. 100) seems not apt because Smiley stressed failure as well as success as his generation saw them. The selection of Wolfe Londoner's election in 1889 as a symbol of the Jews' "general acceptance into the community" (p. 59) appears dubious since Londoner was the only Denver mayor removed from office, for voting swindles that were hardly new in 1889. In general the book presents a useful survey of the saloon in Denver, but seeks only on occasion a historical perspective or context that might have related discrete details to historical social science or conceptual history.

GUNTHER BARTH
University of California,
Berkeley

HENRY C. DETHLOFF and IRVIN M. MAY, JR., editors.
Southwestern Agriculture: Pre-Columbian to Modern.
College Station: Texas A&M University Press, for
the Agricultural History Society. 1982. Pp. viii, 307.
\$23.75.

This collection of papers and comments is the product of a symposium convened in 1980 at Col-

lege Station under the auspices of the Agricultural History Society, the United States Department of Agriculture, and Texas A&M University. Represented among the authors are various academic disciplines (mainly history) as well as administrators and businessmen (but no farmers). The chronological scope of the contents is unlimited—prehistoric to present—and the regional focus is diffuse, inasmuch as the Southwest in this volume includes humid, forested East Texas. So the papers are diverse and too numerous to discuss individually, but taken as a collection, they contribute substantially to several areas of inquiry.

Most notably, the volume illuminates several shadowy aspects of the cattle industry. B. Byron Price, for instance, dismisses all blanket statements about stagnation of ranching in Texas during the Civil War, showing that some ranchers suffered, and others prospered, but none just bided their time. The contributions of A. Ray Stephens on the eastward march of cattle ranching since World War II and of Garry Nall on the development of the feedlot industry on the high plains likewise are fresh works for historians, although they document trends evident to contemporary Texans.

The feedlot business, as J. Roy Quinby points out in the most technical piece in the book, rests on a foundation of hybrid sorghums painstakingly bred by experiment stations. Readers attracted by Quinby's paper will be equally interested in one by Donald J. Cotter on the genetic and economic history of the chile pepper.

Most of the other contributions are concerned with some aspect of the farmer's place in society. Henry J. Shafer, an archaeologist, treats the agricultural climax of Mimbres culture, wherein culture and agriculture were practically synonymous. Robert C. McMath and Bruce Palmer, on the other hand, by offering papers on the origins of Populism, discuss the decline of the status of farming. The decline may account for the paucity of Southwestern farm fiction noted by Richard W. Etulain in another essay. Studies by Leonard J. Arrington, John E. Hutchinson, Gladys L. Bake, Charles C. Colley, and James L. Forsythe pursue subsequent relations between farmers and the government in areas of research, extension, and agricultural policy. For the student of cooperatives and agribusiness, William N. Stokes, Jr., Harold F. Breimyer, Archie P. McDonald, and Ralph S. Newman provide good case studies and commentary.

This anthology occasionally seems in danger of unraveling. Several of the "Comments" sections have nothing to do with the papers that precede them, and the rationale for some of the paper groupings is obscure. The editors, confronting rather than choosing the material they were to publish, no doubt realized this and wisely refrained from

attempting in their introduction to tie the book into a neat package. Yet scholars of the cattle industry, agricultural science, Populism, agricultural policy, and agribusiness will find coherent themes and solid work for their fields in this volume.

THOMAS D. ISERN
Emporia State University

STEPHEN J. PYNE. *Fire in America: A Cultural History of Wildland and Rural Fire*. Princeton: Princeton University Press. 1982. Pp. xvi, 654. \$35.00.

Broad in scope, unprecedented in subject matter, diligently researched and engagingly written, *Fire in America* should be high on the short lists of national book award committees. Stephen J. Pyne is a professional historian and a professional firefighter with fifteen summers' experience on the North Rim of the Grand Canyon. The combination is ideal. Pyne's fascination with wildfire led him to undertake this pathbreaking book. So did a grant from the history office of the United States Forest Service, but, says Pyne, "I was never pressured to write an official history." On the contrary, he frequently exposes the myths in Forest Service dogma regarding fire.

Pyne's basic insight is that wildland fire has more to do with man than nature. For several hundred thousand years, Pyne explains, man has been the primary cause of the modification of the world by fire. Most fires are "anthropogenic"—either caused or allowed to burn by man. When fire is kept out of an ecosystem, man has also been the responsible agent. What this means is that fire and ideas about fire are grist for the historians' mill. Pyne, in fact, calls his book a "cultural," rather than a natural, history.

Simply organizing a subject as vast and neglected as this is an impressive achievement. Pyne begins with the European experience with fire and then focuses on North America. Discussing the Indians, he lays to final rest the romantic notion that "noble savages" did not burn their Mother Earth. Instead, Indians appear to have anticipated the conclusion of fire ecologists regarding the importance of fire to the health of wilderness ecosystems. What the Europeans found was not a "forest primeval" or wilderness, but an environment extensively modified by anthropogenic fire.

Turning to the American experience, Pyne proceeds region by region with a scientist's feel for differences in vegetation (fuel), topography, and climate. He recounts the history of such nationally significant forest fires as the Peshtigo Fire (Wisconsin, 1871) and the Tillamook Burn (Oregon, 1933). Interspersed with the regional histories are speciality chapters concerning fire as a weapon, fire fighting technology, and the history of fire research. A

central theme is the growth of American understanding that the universal suppression of fire (the Smokey Bear syndrome) is unnatural and potentially harmful to the health of ecosystems long accustomed to periodic burning. Fire management, he points out, has come to mean not prevention but intelligent choice of where and when to accept burning.

Fire in America has value for historians of the ecological sciences and of the shaping of the North American landscape. It will interest students of government science and bureaucracy as well as historians of technology. A major contribution is Pyne's discussion of how popular and professional ideas change. Finally, Pyne has explored the history of one of the major components of the American experience—a matter of such transcendent importance to people over time that one wonders how its history has gone unrecorded for so long.

RODERICK NASH
University of California,
Santa Barbara

CRAIG W. ALLIN. *The Politics of Wilderness Preservation*. (Contributions in Political Science, number 64.) Westport, Conn.: Greenwood Press. 1982. Pp. 304. \$27.50.

ROBERT W. RICHTER. *Crucible for Conservation: The Creation of Grand Teton National Park*. Boulder: Colorado Associated University Press. 1982. Pp. ix, 192. Cloth \$12.50, paper \$5.95.

Even the most casual observer should agree that public land use has been central to an understanding of the American West in the twentieth century. Controversy has centered on charges by those closest to the lands of insensitivity to the diversity of local interests and by those living at a distance of the inattention of those living near the public lands to the need to preserve scarce wilderness. Craig W. Allin's *The Politics of Wilderness Preservation* and Robert W. Richter's *Crucible for Conservation* complement each other in considering these issues.

Allin's narrative, which is based largely on secondary sources, printed primary works, and government documents, chronicles the results of the growth of preservation sentiment in the nineteenth and twentieth centuries. He accepts the basic ideology laid out in Roderick Nash's *Wilderness and the American Mind* that preservationist sentiment developed in Europe and America as part of the Romantic movement and that it benefited from Transcendentalism.

While the federal government preserved some wilderness in the nineteenth century, it did so largely because no one pressed for development. By the early twentieth century, however, wilderness

preservation had become a value in its own right. Led by people like John Muir, Arthur Carhart, Aldo Leopold, and Robert Marshall, the preservationists emphasized wilderness as a place with spiritual and recreational values.

Most important in Allin's discussion is the emphasis on shifting alliances in preservation sentiment. At first, preservationists could rely upon railroad companies and the Forest Service for assistance. Those alliances, however, fell apart largely because of the multiple interests involved in transportation and forest land use and the singleminded emphasis on wilderness in the preservation movement. Preservationists replaced such alliances with single-interest associations like the Sierra Club, the Wilderness Society, and the National Audubon Society.

Most of Allin's narrative concentrates on the twentieth century—much of it on legislation. Such acts as the Multiple-Use Act of 1960, the Wilderness Act of 1964, and the National Environmental Policy Act of 1970 required the federal government to consider wilderness values in using federal lands.

Allin's book is also valuable for the case studies it presents in the preservation of wilderness. Cases include the Magruder Corridor and the River of No Return in Idaho, the Boundary Waters Canoe Area in Minnesota, and the various wilderness areas of Alaska. In fact, more than a fifth of the book is about the Alaska issue.

Where Allin's is a general study, Righter's book is most valuable as an extended case study of the creation and expansion of a single national park in northwestern Wyoming. Read from the background of Allin's general work, Righter's title, *Crucible for Conservation*, seems well chosen. Principal opposition to the creation of the park came from ranchers and businessmen in Wyoming, from the Forest Service, and from Idaho irrigators. Major supporters included the National Park Service, certain Wyoming dude ranchers, and the Rockefeller family and interests. To provide land for the park, the Rockefeller family bought private land in northern Jackson Hole. With the creation of the park, the Rockefellers retained about 10 percent of the land as a family ranch; and, through a subsidiary company, Jackson Hole Preserve, Incorporated, became the major concessionaire in the park.

Unlike Allin's more general study, Righter's monograph is based on extensive primary research. His sources include the papers of the principal participants, oral history interviews, the major newspapers, and unpublished sources in the national archives and the federal records centers.

While it is clear that both writers are favorably inclined toward preservation, neither book can be seen as a tract. A balanced presentation does not require the author to be without bias. It does demand, however, that the author give a fair treat-

ment to the point of view with which he disagrees. On this criterion, both authors account themselves quite well—Righter perhaps better than Allin.

Righter recognizes that the creation of the park involved the livelihood of people in Jackson Hole. He understands that a cynic could well conclude that the Rockefeller interests were less than pure since the family ended up with a federally protected ranch and with a concession monopoly. Nevertheless, he argues, quite convincingly, that they have been ideal park neighbors while admitting that most critics find their Jackson Lodge architecturally objectionable. Most importantly, he recognizes that in at least one case, federal officials and conservationists acted underhandedly in trying to cancel Albert Gabbey's stock-raising homestead on Jenny Lake even though the small rancher had complied with the law.

In considering problems connected with conservation and preservation, it is quite important that historians recognize the complexity of the issues involved. It is very easy for preservationists to attribute all opposition to crass materialistic motives without stopping to recognize that many of the people involved are small farmers, ranchers, and businessmen, whose families rely for their modest livelihoods on the use of nearby public lands. On the other hand, those living nearby need to understand that the lands are the common property of all the people of the United States and that any decision as to their disposition must involve the interests of all. Both groups must recognize that the final disposition of such lands will be a political decision and that such a method is the only legitimate way the people can decide how to use their lands.

THOMAS G. ALEXANDER
Brigham Young University

MARILYN TOBIAS. *Old Dartmouth on Trial: The Transformation of the Academic Community in Nineteenth-Century America*. (New York University Series in Education and Socialization in American History.) New York: New York University Press; distributed by Columbia University Press, New York. 1982. Pp. xii, 249. \$35.00.

In 1881 the president of Dartmouth was in trouble with his various constituencies, and, in an unprecedented effort to deal with the problem, the college's board of trustees arranged for a trial that was conducted in New York under the auspices of the New York association of Dartmouth alumni. Using President Bartlett's trial as a point of departure, Marilyn Tobias analyzes with the help of statistical tables the various elements of the Dartmouth community—professors, presidents, trustees, alumni,

and students—in the years 1851 and 1881, her purpose being to demonstrate how those elements responded to a wider world and in the process redefined the nature of the college.

Her conclusion is that the ethos of community had been significantly changed by 1881 as a consequence of a more professional faculty, a more cosmopolitan student body, a less provincial board of trustees, and an urban alumni body seeking formal power in the structure of governance. Now, this pattern does not come as news. It is well presented in many institutional histories and the general histories toward which Tobias is to some extent contemptuous. What is new here is the use of the history of Dartmouth to advance and flesh out certain methodologies and theories of institutionalization and community that are her primary concern.

In offering her study as a model for understanding the complex relationships that are at the heart of the collegiate enterprise, she challenges the reader to compare it with the soundly researched interpretive narratives with which Morison and others have illuminated the history of education and society. The book under review is insensitive to language, repetitious, riddled with jargon, and overburdened with appendixes and pretentious end notes.

Methodological and theoretical purpose sometimes blind the author to the history she is using: because her 1851 period must be tightly communal and organically sound, she does not recognize how divisive were Greek letter fraternities to the Christian community she idealizes. She misses entirely the meaning of alumni seeking formal representation on a board of trustees whose self-perpetuating members are indeed already overwhelmingly alumni. We are told that the Dartmouth professors of 1851 lacked identification with disciplinary peers elsewhere because their definition of community did not require it, whereas it would be enough to conclude that as nonprofessionals they had no peers elsewhere with whom to identify. A change in the academic calendar that resulted in discouraging long absences for students teaching in the district schools is interpreted as a conscious subversion of an older version of community by a more worldly board of trustees, without any recognition of the role of accelerating academic standards in reshaping both the calendar and the community.

This book is offered as a corrective of the view that colleges changed only because they had to adjust to the rise of the university. Among the straw men set up by Tobias, most of them vaguely attributed to a school of simplistic historians from whom she would rescue us, this one deserves no more serious attention than the others.

FREDERICK RUDOLPH
Williams College

FREDERICK C. DAHLSTRAND. *Amos Bronson Alcott: An Intellectual Biography*. East Brunswick, N.J.: Fairleigh Dickinson University Press. 1982. Pp. 397. \$42.50.

Ralph Waldo Emerson called him "the most refined and the most advanced soul we have had in New England." Octavius B. Frothingham, the first major chronicler of the Transcendentalists, rated him second only to Emerson as a leading light of the movement. Yet today the name Amos Bronson Alcott too often inspires only amusing images of an eccentric educator, failed utopian, and befuddled father of Louisa May Alcott. This is the case despite Odell Shepard's amiable if dated biography, *Pedlar's Progress* (1937), and a host of specialized books and articles that have appeared since the publication of Shepard's book. For some time Alcott has been ripe for full-scale reconsideration, and Frederick Dahlstrand's *Amos Bronson Alcott: An Intellectual Biography* takes a major step in that direction.

Dahlstrand's central achievement is his coherent and sensitive exposition of Alcott's intellectual growth. He traces the entire journey from 1799 to 1888, from Alcott's years as a peddler through his patriarchal stewardship of the Concord School of Philosophy. The author makes some attempt to place Alcott's thought in the context of more general social and economic developments. The main strength of this account, however, is its close and finely textured reading of the massive manuscript collections. Dahlstrand finds that "at the root" of Alcott's ruminative ponderings lay the question of "the relationship between human beings and their environment" and "the consequences of human interaction with the forces of nature" (p. 12). In this sense he contemplated some of the same questions central to the work of Kant, Hegel, and Fichte and anticipated, according to the author, the radical empiricism of William James.

Dahlstrand's decision to detail even the less well-known and seemingly "less important" chapters of Alcott's life reinforces the basic value of this volume. He gives us a fascinating account of the post-Civil War years, ones that saw Alcott lecture extensively in the West, come to grips with Darwinism and other scientific philosophies, and reassess his life and philosophy. By taking seriously the later years, Dahlstrand allows us to see the more famous episodes in Alcott's life—the Temple School, Fruitlands, and the "Conversations"—as part of an ever-evolving vision.

If there is a problem with the book, it is that the author defines the task of "intellectual" biography too narrowly. One often becomes lost in the thickets of Alcott's journals, needing some more definite portrait of the man who is writing them. All the biographical material is present, but somehow Alcott seems a more distant figure than that of Odell

Shepard's otherwise far less useful biography. A more acute sense of the person might have brought us closer to understanding the nature of the Alcott, that most refined and most advanced soul, his contemporaries so respected.

No matter, for in most other ways Dahlstrand has taken us far with his account of Alcott's lifelong meditation. He has also made a significant contribution to the recent literature reinterpreting the roots and nature of Transcendentalism.

ROBERT H. ABZUG
University of Texas,
Austin

ROBERT M. CRUNDEN. *Ministers of Reform: The Progressives' Achievement in American Civilization, 1889-1920*. New York: Basic Books. Pp. xii, 307. \$17.95.

Robert M. Crunden sees this book as an expansion of his earlier study, *A Hero in Spite of Himself: Brand Whitlock in Art, Politics, and War*. To accomplish this, he has examined the lives of a hundred individuals born between 1854 and 1874 and discusses twenty-one in some detail. He believes that these individuals shared a common cultural background. They were Anglo-Americans influenced by the heritage of New England and raised in a family environment of strong Protestant moralism. He argues that Progressivism can best be understood as a religious revival, inspired by Protestantism, but expressing itself in the more secular language of civil religion. At first glance, Crunden's book appears similar to *No Place of Grace* by T. Jackson Lears in exploring the psychological tensions of a cohort at the turn of the century. Crunden writes that Progressivism was a response to the problem of "how a young person could find a meaningful place for himself in a world that seemed hostile to every creative impulse and deep-seated need" (p. xii). But unlike Lears, who stressed the psychological burden that the values of rationality, efficiency, and bureaucratic organization placed on the Protestant heritage of the Anglo-American elite, Crunden focuses on the problem of the decline of an officially Protestant culture that made it impossible for many creative young people to choose careers as Protestant ministers. Instead they poured their energy into developing new professions where they could express their Protestant consciences. He argues that Protestant moralism was the motivational force for those many academicians, among them John Dewey, who could no longer associate themselves with any Protestant denomination.

Crunden calls this pattern "innovative nostalgia." He has two major chapters on its expression in the arts. Progressive artists, for him, put new subject matter into the artistic forms inherited from their

parents. The novelists, poets, and painters failed to create great art, he argues, but Charles Ives in music and Frank Lloyd Wright in architecture broke the older forms to accomplish the greatest achievements of any American in those artistic mediums.

He then explores the role of the heritage of Protestant moralism in the Progressive party of 1912 and in Woodrow Wilson's foreign policy. He sees the religious enthusiasm of the Progressive party as the end of an era because "The country was soon too pluralistic and the force of Protestant religion too weak for such a thing to happen again" (p. 224). But Crunden's thesis that the Progressives were a unique generation because of their "innovative nostalgia" may need to be qualified. Sacvan Bercovitch, in his recent book, *The American Jeremiad*, has argued that there has been a continual rhetorical ritual in Anglo-American culture from the Puritans to the present. Participants in this culture, for him, always judge the present as a decline from the promise that Americans are a chosen people. But the rhetorical ritual reassures them that the original promise will be fulfilled in the future as current problems give way to progress. If Bercovitch is right, "innovative nostalgia" has been a characteristic of generations both before and after that of the Progressives.

DAVID W. NOBLE
University of Minnesota,
Twin Cities

SUSAN TIFFIN. *In Whose Best Interest? Child Welfare Reform in the Progressive Era*. Contributions to the Study of Childhood and Youth, number 1.) Westport, Conn.: Greenwood Press. 1982. Pp. 310. \$29.95.

W. NORTON GRUBB and MARVIN LAZERSON. *Broken Promises: How Americans Fail Their Children*. New York: Basic Books. 1982. Pp. x, 358. \$20.75.

Finley Peter Dunne, the American humorist, once made his fictional Irish character Mr. Dooley say something to the effect that we are all in favor of reform "but, on the other hand, not so fast." Both books under review impressively document Dooley's point. They are concerned with a central dilemma of the American progressives and of American liberals down to the present: how to propose reforms that seem necessary but that threaten institutions and systems on which the reformers themselves depend for status and power. The liberal solution to this dilemma—to be unaware of it or somehow to live with the ambiguities it has created—is well illustrated by American efforts to come to terms with the problems of children. Children are relatively powerless as a group; hence our ambiguities about reform are clearly revealed at the point

where our own power and class interests collide with our stated aspirations for all children.

Both studies describe the progressive-liberal dilemma effectively. Susan Tiffin's *In Whose Best Interest* is concerned with child welfare reform in the Progressive era. Arguing correctly that most recent writers on child welfare have concerned themselves with juvenile delinquency or child labor, she concentrates on efforts to deal with dependent and neglected children during the period 1890–1920. Interested neither in a legislative history nor in one dealing primarily with social work techniques, the author sets out to cover “a spectrum of activity, including campaigns for new laws, changes in the structure of the welfare system, and the adoption of new methods of care and standards of treatment” (p. 6). Tiffin, a member of the history department at the University of New South Wales, has succeeded in writing a solid, well-documented descriptive history based on government documents, reports of welfare agencies, and secondary materials. She presents a profile of children and of the child-savers and devotes major attention to foster and institutional care, illegitimacy, juvenile courts, and the Children's Bureau. The author concludes that “if much of the activity on behalf of dependent and neglected children had beneficial results, it is also clear that much was undermined by the ambivalent positions of social workers and philanthropists” (p. 285). Tiffin's work lacks the drive and sustained argument in support of a thesis that engages one's full attention. An overzealous editor has chopped the text up into far too many subsections.

In contrast to Tiffin, W. Norton Grubb, an associate professor of economics at the University of Texas, and Marvin Lazerson, professor of education at the University of British Columbia, are arguing a thesis from the very first page. *Broken Promises* is a lively account of how the progressive-liberal dilemma has prevented a truly unified and effective approach to the problems of the nation's youth. Its primary focus is on the post-World War II era and on present and future policy directions. The authors assert that the doctrine of *parens patriae*—the state should intervene in child-rearing only when families have failed—“has . . . limited and distorted public programs for children” (p. 6). We have also broken promises to children by our “collective failure to overcome the inequalities that affect their lives” (poverty, racial problems, the class divisions of a capitalist economy, other structural inequalities [p. 6]). These realities are further documented in chapters on the public schools, youth employment programs, the juvenile justice system, welfare policy, and child care. Grubb and Lazerson conclude that we must stop making the family a scapegoat for problems that are economic, recognize that the family is no longer private, and extend

family policy, by means of a radical analysis of American class divisions, to the point where we “begin the politically difficult process of remedying the divisions of class through collective action” (p. 268). We need, further, to work for “full employment, a decent welfare system, and the elimination of racial discrimination” (p. 297). Until we “revise our fondest myths about children and our most deeply-rooted institutions” (p. 126) we will continue to see the frustration of children's interests by the limitations of *parens patriae*, self-interest, and national structural inequalities.

Broken Promises is a well-written book, although the authors tend to be repetitive and to preach. The reader wearies of their continual reiteration of the facts that state capitalism has resulted in structural inequalities, that the middle class can work the system better than the poor, and that somehow our present is a product of our past history. They offer a pristine liberal agenda for the future (for example, “We can give nothing to young people if we do not offer a world in which adults can live decent, productive, and rewarding lives, in which they can work and love in satisfying ways” [p. 184]); like Tiffin, they show how very much we still live in the Progressive era.

It would have been better if all three authors had allowed the children themselves to speak. If they had, we might have gained a better understanding of the inherent ambiguity of all lives and of all periods in human history. Understanding the great price that must be paid for survival, let alone “progress,” in any historical time, we could then go on to appreciate the inevitable connection of self-interest with truly viable reform.

JEREMY P. FELT
University of Vermont

DAVID ROSNER. *A Once Charitable Enterprise: Hospitals and Health Care in Brooklyn and New York, 1885–1915*. (Interdisciplinary Perspectives on Modern History.) New York: Cambridge University Press. 1982. Pp. ix, 234. \$29.50.

The emergence of the modern hospital traditionally has been associated with the medical revolution of the late nineteenth century, that is, the discovery of bacteria and the introduction of anesthesia and aseptic measures that transformed surgery. While conceding these points, David Rosner stresses that the nature of hospitals changed around the turn of the century largely in response to altering demographic and social conditions. Hospitals were originally small charitable institutions designed to provide medical care and moral uplift for the deserving sick poor, with the trustees and patrons usually reserving the right to determine which patients

could be admitted. With the introduction of urban transportation, old neighborhoods disintegrated and the upper classes moved to the suburbs. Thus the inner city hospitals found themselves losing patrons while facing an increasing number of poor seeking admission. At the same time improvements in medicine were raising the cost of medical care. Out of necessity, hospital administrators increased the number of beds for private patients and began charging at least nominal fees to those on charity. This situation was greatly aggravated by the depression, which began in 1893. As economic conditions worsened, hospitals, particularly those for women and children, found themselves flooded with requests for admission.

Faced with deficits, hospital trustees, who had traditionally considered themselves stewards for the poor, now began to look upon them as a burden. Forced by financial need to rely on paying patients, the trustees recognized that they had to ally with private physicians. The latter were soon taking a major role in hospitals and before long were questioning the authority of the trustees and administrators. In New York, as in many other cities, hospitals, as charitable institutions, frequently received state and municipal funds. The Progressive movement, with its corollary of professionalism and efficiency, modified the way funds were granted to hospitals, the net effect of which was to further strain the financial condition of small hospitals. In addition, physicians used their growing political strength to undermine the dispensaries that provided much of the medical care for the lower income groups.

Rosner's book deals exclusively with New York City with the exception of the chapter, "The Battle for Morningside Heights." This one details how the trustees of New York Hospital fought off developers for many years and, when they finally had to give in, ensured that the land they controlled would be turned into middle-class housing. Other than this chapter, Rosner's thesis holds true for all American urban areas. In his conclusion Rosner notes with regret that hospitals within a relatively short time began catering to the well-to-do, turning away the indigent, and establishing a caste system within their doors. Scientific medical care geared to those who could afford it replaced the social services offered by earlier hospitals. The author provides some fascinating details on New York hospitals, and his book is of major significance in understanding the evolution of the modern hospital.

JOHN DUFFY
University of Maryland,
College Park

HARRY F. DOWLING. *City Hospitals: The Undercare of the Underprivileged*. Cambridge: Harvard University Press. 1982. Pp. vii, 245. \$22.50.

Believing that public hospitals today need a sense of their own history to plan for the future, Harry F. Dowling examines in this book the history of public care for the sick poor in America. Beginning with the early almshouses and examining the "practitioner period" of the second half of the nineteenth century and the "academic period" of the first half of the twentieth century, Dowling tells a story of dedicated hospital personnel fighting against insufficient funds, inadequate facilities, and exploding public need. Dowling admits that "doctors themselves are sometimes the problem" (p. 3) but points the finger of blame for the second-class medical care available to the poor most often at the politicians, whose self-interest, Dowling posits, kept public spending to a minimum.

Dowling selects city hospitals in Baltimore, Boston, Chicago, and Washington, D.C., as prototypes through which to examine the history of public hospitals—largely because he was associated with each of them during his own medical career. He believes that his personal experience "brought knowledge and insights that could not have been obtained in any other way" and gained him unique access to sources and personal interviews (p. 3). Dowling's experience among the overworked and underpaid medical staffs of public hospitals leads him to an insider's bias about the source of hospital problems and, indeed, his most interesting chapters are the two that cover the period since the 1930s of his own participation. But Dowling's experience is limited to the most recent past and does not serve him particularly well for examining the earlier periods, either for providing unique sources or for understanding a system based less on the educative functions of clinical experience than on evolving ideas of public responsibility.

Dowling believes that while politicians, such as James Michael Curley in Boston, sometimes may have supported public care for the sick poor, most often they were not inclined to generosity. Rather, Dowling credits medical schools, which realized that affiliation with public institutions improved the quality of medical education, as the saviors of public hospitals in the twentieth century. But Dowling admits that city hospitals today still provide inadequate public care. He thinks current problems can be solved (leaning on lessons from the past) by developing closer ties with single medical schools, forming public corporations to administer city hospitals, and including community representatives on governing boards. Dowling, who has spent his life working within the system characterized by "the undercare of the underprivileged," ends his book with a humanitarian plea: "Does anybody care?"

The book is well researched and, although limited to the institutions in which Dowling himself worked, probably fairly representative. The large span of

time covered leads to some unsubstantiated generalizations, especially about social and political influences outside the hospital. The practitioner bias—which Dowling does not deny or hide—provides both fresh insights and some limiting perspective; it makes parts of the book more valuable as a primary source than as a secondary analysis. Historians will find the book useful even though it is directed explicitly to practitioners and hospital administrators and not to scholars.

JUDITH WALZER LEAVITT
University of Wisconsin,
Madison

GEORGE JUERGENS. *News from the White House: The Presidential-Press Relationship in the Progressive Era*. Chicago: University of Chicago Press. 1981. Pp. x, 338. \$25.00.

George Juergens offers a fully documented overview, based on extensive reading, of changing relations between the office of the presidency and the Washington press corps during the administrations of Theodore Roosevelt, Taft, and Wilson. One chapter discusses George Creel's Committee on Public Information; another includes a careful summary of Roosevelt's 1908 attempt to silence Joseph Pulitzer for publishing details about presidential involvement in the Panamanian revolt of 1903. In general, Juergens feels Roosevelt did the best job, though Wilson had a way of communicating directly with the American people that even TR lacked. Taft did just about everything wrong. Juergens believes that in the modern world, news, "and the way it is presented, does far more to mold public opinion than editorials" (p. 7). A modern president needs to create news, not settle for a "daily chronicle of official activity" (p. 7). Finally, a successful president must shape the news, creating an image of leadership, rather than just lead.

News from the White House contains a number of amusing anecdotes and some common-sense judgments but is not a profound work. Historians will find nothing new in lengthy summaries of the Payne-Aldrich Tariff, the Ballinger-Pinchot affair, or Wilson's problems at Versailles. They will be skeptical of unsupported statements that effective cultivation of reporters affected the outcome of such episodes, or could have done so. As George Reedy has argued, image politics is "effective only in regard to adjectives, not to the hard substantive news that is the ultimate shaper of public opinion." Juergens disagrees but fails to make his case. Instead of a conclusion about changing power relations among the media and existing political institutions in this society, or an analysis of whether the quest for image is detrimental to what a president should

spend his time doing, Juergens ends with some modest remarks about how lucky we are that a free press is ready to report mistakes of those in office, even though it makes presidents mad. If government silenced the press, he declares, "everybody would be the loser" (p. 272).

Students of journalism will probably be most interested in this book, though they will continue to turn to Pollard's *The President and the Press* and John Morton Blum's marvelous study of Wilson's aide, Joseph Tumulty. Those who teach survey courses will want to use Wilson's threat to give a press photographer "the worst thrashing you ever had in your life; and what's more, I'm perfectly able to do it" (p. 133); or Roosevelt's advice to Taft about what he allowed photographers to record: "You never saw a photograph of me playing tennis. I'm careful about that; photographs on horseback, yes; tennis, no. And golf is fatal" (p. 101).

DAVID CULBERT
Louisiana State University

ARTHUR S. LINK, editor. *Woodrow Wilson and A Revolutionary World, 1913-1921*. (Supplementary Volumes to the Papers of Woodrow Wilson.) Chapel Hill: University of North Carolina Press. 1982. Pp. viii, 241. \$23.00.

This important book consists of seven papers that were first presented in October 1979 at an international symposium at Princeton University sponsored by the Woodrow Wilson Foundation. Collectively, these excellent essays survey, synthesize, and advance the work on Wilsonian diplomacy developed over the last two decades. The Wilson who emerges is a figure of great complexity, sensitive to national and historical particularities, yet anxious to project American liberal values toward the achievement of a more universal world order.

The volume opens with essays on Wilson's response to the two great revolutions of his time: Lloyd C. Gardner on "Woodrow Wilson and the Mexican Revolution," and Betty Miller Unterberger on "Woodrow Wilson and the Russian Revolution." Of the two pieces, Gardner's is the more balanced and valuable. Gardner captures Wilson's central dilemma concerning revolutionary Mexico: how to resist the efforts of foreign powers and American business interests to use intervention to turn the Mexican clock back to the old autocratic order, while being able, simultaneously, to curb radical excess and to help guide Mexico to a progressive fusion of democracy and capitalism. Gardner presents a Wilson both attracted and repelled by the possibilities of the first Third World revolution of the century.

For her part, however, Unterberger misses the similar tension in Wilson's approach to the Russian Revolution. She accurately portrays Wilson's distaste

for military intervention in Russia, his rejection of Allied plans either for the re-creation of the eastern front or for an anti-Bolshevik crusade, and she carefully documents Wilson's persistent efforts to check Japanese imperialism in Siberia. What Unterberger does not sufficiently convey is the side of Wilson that was explicitly or implicitly anti-Bolshevik. For example, she glides past the ways in which the Fourteen Points address and the Prinkipo proposal represented diplomatic efforts to turn the Bolsheviks away from their revolutionary extremism and toward pro-Allied resistance to Germany in the first instance and democratic pluralism in the second instance. It is true that Wilson lacked Lansing's absolute clarity as to the extent to which Bolshevism promised to destroy all the positive values of the democratic Russian Revolution, but Wilson was much closer than Unterberger lets us see to an understanding that Bolshevism was not congruent with the Russian people.

Space does not permit me to deal fully with the other essays, but it should be noted briefly that Kay Lundgreen-Nielsen summarizes well Wilson's approach to the territorial and political complexities involved in "The Rebirth of Poland," and Inga Floto's essay, "Woodrow Wilson: War Aims, Peace Strategy and the European Left," deals thoughtfully with Wilson's ambivalent approach to those elements in the liberal and democratic socialist movements that wished to champion his peace program militantly. In his "Woodrow Wilson and World Order," Kurt Wimer carefully describes the ways in which the essentials of Wilson's eventual peace program were present in his pre-1917 mediation efforts, and Herbert G. Nicholas, in his essay, "Woodrow Wilson and Collective Security," brilliantly develops the complex way in which Wilson's idea of the League of Nations sought simultaneously to preserve and yet also to transcend the power political implications of a world alliance for collective security. In the concluding essay, "Reflections on Wilson and the Problems of World Peace," Whittle Johnson searches for a synthesis on Wilsonian diplomacy by exploring those elements in domestic and world politics that stood between Wilson and the achievement of his vision of liberal world order. The question remains, however, whether or not Johnson recognizes sufficiently the extent to which traditional or power political concerns coexisted with idealistic and moral concerns in Wilson's own mind.

N. GORDON LEVIN, JR.
Amherst College

YVES-HENRI NOUAILHAT. *France et États-Unis, août 1914-avril 1917*. (Série Internationale, number 10.) Paris: Publications de la Sorbonne. 1979. Pp. 484.

Almost twenty years ago two of J.-B. Duroselle's most able protégés at the Sorbonne undertook to investigate Franco-American relations during the First World War. André Kaspi's magisterial account of the flawed alliance between the two nations during the period of American belligerence appeared in 1976 to almost universal acclaim. Yves-Henri Nouailhat, who concentrates on the years of American neutrality, has met an equally high standard of resourceful scholarship and clear exposition. Together, these works brilliantly illuminate the differences of temperament and outlook separating France and the United States. They go far to explain why during the 1920s animosity on a popular as well as on a governmental level could so easily dissipate the residual feeling based on wartime comradeship. Both books will stand as essential building blocks for all future studies of American foreign policy during the wartime era.

Nouailhat's material will seem largely fresh, even to specialists. The best of the explicitly multinational analyses of Wilsonian diplomacy, such as Ernest May's *The World War and American Isolation*, focus on England and Germany. They tend to ignore France. This emphasis, of course, properly reflects the weight of documentary evidence. Wilson and his State Department bent their primary efforts toward preserving American neutral rights from infringement by German submarines or British blockade forces. While the French took yet a tougher theoretical stand against unimpeded neutral commerce than did the British, the latter had the ships to enforce the policy and, along with the Germans, attracted the bulk of White House attention and animus. Similarly, the endeavors by Wilson and House to arrange a peace without victory depended above all on winning British Liberal acquiescence. The French, it became clear during the first House mission, wanted no American meddling, considered the notion of a compromise on Alsace-Lorraine particularly abhorrent, and would have to be dragged along late in any scheme for diplomatic intervention. While Nouailhat concedes the subordinate role played by France in Wilson's struggle to preserve neutral rights and to impose his own vision of world order, he nevertheless brings to bear new material on these issues from French sources and chronicles events from a useful perspective.

Nouailhat exhibits particular skill in proving how little France and the United States interacted before 1914. The French sent few emigrants and little capital to the United States; mutual tariff barriers kept trade links slender; and the cultural ties scarcely penetrated beyond restricted circles on the East Coast. Americans generally considered the French decadent—sexually incontinent, antireligious, and lacking in aptitude for hard work. The French,

when they thought of Americans as anything but stock figures to make points about their own culture, reciprocated this disdain, holding the Yankees to be materialistic, deficient in refinement, and beset by feminist militancy. Nouailhat discerns signs of rapprochement just before the war but lauds Ambassador Jean-Jules Jusserand for furthering the process after the outbreak of hostilities.

Jusserand argued that the Allied cause would speak for itself, and through 1916 he prevailed against Berthelot and others at the Quai d'Orsay who favored a crude propaganda campaign in the United States on the German model. Nouailhat shrewdly concludes that such reticence did much to cultivate the popular sentiment that France fought primarily for the right, as opposed to the practical aims said to animate England. He neglects, however, to dwell on the weak feature of Jusserand's stewardship of the embassy, namely his inadequacy in dealing with questions of finance. Despite discretion on that point, Nouailhat makes particularly effective use of French war and finance ministry records to show how France became, by 1916, totally dependent on American supplies and loans. He gives too much credence, perhaps, to Octave Homberg's strictures against J. P. Morgan & Company's failings as France's banker and purchasing agent. Yet he demonstrates with irrefutable elegance just how close France came to financial catastrophe when Wilson turned off the financial spigot in connection with his "peace offensive" of December 1916.

Nouailhat deals in exhaustive fashion with the legal issues of blockade, the intricacies of materiel procurement, the subtle evolution of public opinion, and the larger questions of diplomacy. He has cut this book to half the length of the *doctoral d'état* published in 1977 in a limited offset edition, but even the most exacting specialists will find it sufficiently detailed for all purposes, and excessively full for some. Nouailhat has not liberated himself entirely from superciliousness toward America: at one point he reminds readers gravely that the United States does not figure merely as a country of "more or less rapacious businessmen" but also contains "an intellectual elite, often idealistic and conscious of a great historic and spiritual debt toward the country of La Fayette" (p. 424). Possibly a few such statements will win for this fine study added credibility in France.

STEPHEN A. SCHUKER
Brandeis University

LESLIE FISHBEIN. *Rebels in Bohemia: The Radicals of The Masses, 1911–1917*. Chapel Hill: University of North Carolina Press. 1982. Pp. xv, 270. \$24.50.

In reviewing the 1936 Granville Hicks biography of John Reed, Max Eastman claimed that the kind of people who become college professors are incapable of understanding the freewheeling radicalism of a man like Reed. A debatable proposition, perhaps, but one that seems to hold true for Reed's generation, which after more than half a century still awaits a comprehensive historical accounting. Leslie Fishbein's book is yet another attempt to make sense of that subculture of bohemians, radicals, artists, and intellectuals who made Greenwich Village such a lively place between 1910 and 1917. Better than any of its predecessors, *Rebels in Bohemia* clearly and in great detail lays out the beliefs of this generation on such topics as sex, religion, feminism, blacks, immigrants, art, class struggle, and life style. But while the author broadens our knowledge of ideas, she shows less insight into the lived experience of her subjects. The result is half the book we really want, a work that generally ignores the fact that all people change and develop over time.

Devotion to intellectual analysis does not mean that Fishbein is especially perceptive or well informed. Certainly too much scholarship has been done on Freudian theory in recent years to allow psychoanalysis to be once again caricatured as a kind of simple-minded process of "psychological adjustment" to capitalist reality. Nor is the author much better with socialism. Accepting *The Masses* self-definition as a socialist magazine, Fishbein proceeds to give this her own particular gloss. For her, socialism is a clearly defined program with aims that must be achieved in a disciplined, revolutionary manner. This point of view allows her to spend a good deal of time berating her subjects for frivolity. To show a deep interest in Freud, or love affairs, or drugs, or lyric poetry seems to indicate—in her estimation—a lack of seriousness about changing the economic system.

Well, perhaps. But this is to put the whole issue backwards. For many of *The Masses* crowd (which *Rebels in Bohemia* never defines in any precise way, and which seems to include almost anyone who ever peered into an issue of the publication), socialism did not precede other interests, and certainly it was not the touchstone against which they measured beliefs or behavior. Some, like John Reed, found socialism "dull as dishwater" until the Bolshevik Revolution; many thought it far less interesting or valid than anarchism. The glory of Greenwich Village in the teens was its openness to new forms of political, social, and artistic expression. To understand how these fit together, the historian of bohemian rebels must come to grips with the fact that men and women struggle, laugh, love, and create works of art in time, and not between the covers of books, that their ideas are part of the fabric of

existence and not separable into formal analytic categories. Fishbein may provide a wealth of useful information, but *The Masses* crowd still awaits a historian that will make it come to life.

ROBERT A. ROSENSTONE
California Institute of Technology

WILLIAM H. HARRIS. *The Harder We Run: Black Workers since the Civil War*. New York: Oxford University Press. 1982. Pp. ix, 259. \$17.95.

There has been a small but distinguished line of general treatments of the history of black workers in America: Charles H. Wesley, *Negro Labor in the United States, 1850-1925* (1927); Sterling D. Spero and Abram L. Harris, *The Black Worker* (1931); Horace R. Cayton and George S. Mitchell, *The Negro Worker and the New Unions* (1939); F. Ray Marshall, *The Negro and Organized Labor* (1955); and Philip S. Foner, *Organized Labor and the Black Worker, 1619-1973* (1974). Joining that list now is William H. Harris's *The Harder We Run: Black Workers Since the Civil War*. Harris offers us an articulate and perceptive overview of the blacks' struggle for opportunity and equality in the American work place. His is not a tame account. The history of black workers, Harris argues, "is the history of discrimination, not to say downright oppression" (p. 3).

Starting off with a discussion of the legacy of slavery, Harris masterfully weaves his way through one hundred twenty-five years of labor strife in which blacks, whites, and industrial America were the principal protagonists. He pays special attention to the racism of white unions and industries, the transition of blacks from rural and agricultural labor to urban and industrial work, the efforts by black workers to organize, the impact of the Great Depression, World War II, and the civil rights movement on black labor and the work place, and the significance of the Bakke and Weber decisions to the future course of affirmative action in training and employment opportunity.

Harris's conclusion about the future of black workers in America is pessimistic but realistic. He argues, in philosophical harmony with Sidney M. Willhelm, *Who Needs the Negro* (1970), that black workers have, at best, made only marginal progress in acquiring meaningful power and a viable place in the modern American economic system. Says Harris: "At the beginning of the 1980s, after more than a century of struggle, little had been done to limit the impact of race on employment and income" (p. 189).

The Harder We Run, like any attempt at a comprehensive survey, is not without its flaws. The scholar looking for new insight into the history of black

labor will probably have to look elsewhere. Neither is the work completely comprehensive, of course. Inevitably, in a work of the type that Harris has attempted, the rule of some people and organizations important to the question at hand will be minimized or overlooked. Moreover, there is a tendency in labor studies to neglect the cultural arena, and Harris's study is no exception. He does not, in this book, explore beyond the concept of traditional industry. There is no discussion, for example, of the tremendous struggle waged by black "workers" in the performing arts. There is no mention of the Colored Musicians Union or the gallant fight by black musicians to integrate the American Federation of Musicians. Black performing artists battled against discrimination in hiring, unfair pay scales, and bad working conditions like their counterparts in the more traditional industries.

Despite these shortcomings, *The Harder We Run* is a valuable synopsis of the story of black workers. It is an important contribution to African-American and labor history. We need more historians like William H. Harris who are willing and able to produce scholarly overviews that are serviceable to both the academic community and the public at large.

DONALD SPIVEY
University of Connecticut

ARNOLD SHANKMAN. *Ambivalent Friends: Afro-Americans View the Immigrant*. (Contributions in Afro-American and African Studies, number 67.) Westport, Conn.: Greenwood Press. 1982. Pp. xiv, 198. \$25.00.

Most peoples of the world have congratulated themselves on the superiority of their own kind, and one of the great "proofs" to "superior" peoples of the rightness of their pronouncements on race has been that "inferiors," when given the chance, have operated similar racial hierarchies. Thus the Victorians were pleased to reflect that light-skinned Indians and Africans looked down on their darker-skinned brethren. And in the United States, diverse evidence has been produced to demonstrate the universality of race prejudice. Native Americans have been presented in conflict with black Americans; Afro-Americans pale enough to "pass" have been said to despise the blacker members of their group; while the early feminists, though coming to women's rights via abolitionism, have been reproved for succumbing to racism. When smugness or malice are set aside, we can find complex reasons for this depressing, if easily misrepresented, phenomenon.

We should therefore not be surprised by the findings of Arnold Shankman's survey of Afro-American responses to non-European, Jewish, and

Italian immigrants, in the West and South respectively, between the 1880s and 1930s. Indeed, this interesting study presents so much evidence of mutual hostility and misunderstanding that it seems inappropriate to talk about friendship, "ambivalent" or otherwise, except between Southern blacks and Jews. There were objective causes for mistrust. Blacks and immigrants each feared the economic competition posed by the other. The newcomers' religious beliefs were usually unfamiliar and unappealing to blacks, who also resented the fact that many immigrants received better treatment than they themselves did from whites. Particularly obnoxious was any indication that, for self-preservation or out of conviction, the immigrants were adopting white American antipathies toward blacks.

Antagonism did not always prevail. Immigrant entrepreneurs might welcome black custom without exploiting it, and black spokesmen could hold up for emulation the thrift, community solidarity, and race pride of the Japanese and Jews. Yet it is apparent that blacks, while condemning discrimination against any minority, entertained derogatory, generalized images of some of the strangers. Why they did so is less clear. The author suggests that ignorance of Oriental cultures was a key factor and that racism provided a means by which blacks might identify with the white majority. But because *Ambivalent Friends* is the companion volume to a forthcoming work on black attitudes to immigrants in the North, Shankman hesitates to "make many firm judgements" (p. xiii). He does, however, raise a point that seems worth developing more fully: namely, that the middle-class black press on which his book so heavily relies represents only one section of black opinion. Since newspapers invariably offer the most extreme expression of a society's racial thinking, it would be helpful, perhaps with the aid of additional oral history materials, to know more about the routine relations between the blacks and immigrants. Another explanation for the disappointing level of cooperation between them may have been that, in the period studied, Afro-Americans and immigrants alike were still struggling to entrench their separate protest and protection organizations.

CHRISTINE BOLT
University of Kent

JOSEPH P. SCHULTZ, editor. *Mid-America's Promise: A Profile of Kansas City Jewry*. Kansas City: Jewish Community Foundation of Greater Kansas City or American Jewish Historical Society, New York. 1982. Pp. xvii, 405.

WILLIAM TOLL. *The Making of an Ethnic Middle Class: Portland Jewry over Four Generations*. Albany: State University of New York Press. 1982. Pp. xii, 242.

Both Portland, Oregon, and Kansas City, Missouri, are regional economic centers, and each had a mainly German Jewish population from its early days. The mass immigration of East European Jews raised Portland Jewry's numbers to a peak of ten thousand around 1926, and they now stand at eighty-seven hundred; Kansas City figures have consistently been slightly more than twice as high as Portland's. Each community has just been added to the lengthy roster of American Jewish communities whose histories have been written. But the histories themselves differ radically.

Mid-America's Promise consists of studies that mean to present a "profile" of Kansas City Jewry. The book concentrates on the organized community's affairs, in its synagogues and philanthropies and schools and community center, and discusses community newspapers and support for Israel. The chapters, while laden especially with oral material, are short of ideas or analysis and are likely to stir little interest outside Kansas City. But there are gleams of light. Federal Judge Howard F. Sachs, surveying the place held by Jews in the city's life, finds cycles of acceptance and rejection. Sharon Lowenstein's study of the arrival and settlement of Jewish immigrants in the city, mainly since 1933, weaves together admirably Jewish communal policies, the needs of clients, and the requirements of the law. Only one contributor, David M. Katzman, seems aware that a hierarchy of wealth, prestige, and power exists within a voluntary community, in his forceful examination of "The Origins and Ascendancy of the Federation, 1933-1946." The book takes very little notice of nineteenth-century Kansas City Jewry, economic and demographic data are sparse, and its many pictures do not illustrate very much. An institutional, uncritical perspective prevails.

William Toll, in contrast, has written a lean, intellectually powerful study of Portland Jewry by the methods now used in American social history. He draws heavily upon recent writing in that field, and upon American Jewish local histories as well, for analysis and comparison, while conveying to the reader the sense of newness and the lack of binding precedent that pervaded Jewish life in Portland even among Jews who sought to follow the ways of their ancestors. Faithful to his title, Toll is more interested in the Jews of Portland as individuals and families than in their communal and institutional life. He properly emphasizes the transiency of the early, mostly male Jewish population, who were frontier businessmen seeking opportunities. But once settled and successful, individual Jewish businessmen could make their mark in public life, since hostility to Jews was minimal before the 1920s and the city had no massive, domineering corporate interests. Toll links Jewish participation in Port-

land's and Oregon's public life with Jewish communal life by means of the interesting suggestion that their common purpose was to establish a stable civic order. But this does not go far enough in explaining the roots of ethnic-religious loyalty that underlie a voluntary community. In fact, the process of building a community is almost disregarded. The author is a trifle doctrinaire, it seems to me, in categorizing much of the local charitable and educational endeavor as expressions of the Jewish woman's expanded world after her liberation from the patriarchal family of traditional European Jewry. In that society, women were typically active in charitable work, frequently conducted businesses, and had plenty to say in family matters; America probably represented a broadening, not a liberation. Some of Toll's most valuable data concern fertility, which show its sharp decline among native Jewish women. Data concerning Portland's B'nai B'rith members, however, are presented to excess, and pace Toll, they are not fully representative of all the city's Jews.

Toll only hints at the connections between Jewish beliefs and practice on the one hand and social class and European background on the other hand. He hardly examines religious ideologies, and, while he says much about the Portland Hebrew School, its educational ideals are little noticed. Ideas, both latent and articulate, were required to found and maintain a Jewish community, just as more than inertia was needed for Jews to remain Jews. Thus, the Jewish community as an entity remains rather shadowy.

Portland Jewry must have what we have learned to call an *histoire événementielle*, but it hardly appears. Events must have occurred, but census schedules and even private papers and institutional minutes seldom reveal them, and Toll does not cite the press at all.

The Making of An Ethnic Middle Class is a truly original book, which fully merits close reading and informed debate because it raises by example basic questions concerning the method and content of local Jewish history and does so with vigor and intellectual independence. It should be recognized as an important work in American Jewish historiography, and it deserves to exert wide influence.

LLOYD P. GARTNER
Tel-Aviv University

DANIEL SCHAFER. *Garden Cities for America: The Radburn Experience*. Philadelphia: Temple University Press. 1982. Pp. xiv, 276. \$25.00.

In 1929 in Bergen County, New Jersey, a remarkable group of urban planners embarked on the most innovative housing experiment undertaken in the United States between the two world wars,

Radburn community. Inspired by the garden city concept of Ebenezer Howard, the City Housing Corporation, the corporate arm of the Regional Planning Association of America, constructed what it believed was a model for future urban development in the United States. Radburn was the culmination of a decade of work by the Regional Planning Association, which since 1923 had functioned as a loosely organized study group committed to awakening American city planners to a new vision of urban development. Consisting largely of New Yorkers, the RPAA advocated an ecological approach to urban growth that viewed cities as parts of larger human and natural systems. Effective planning included comprehensive regional studies; control of automobile and rail transportation; integration of housing, manufacturing, shopping, and cultural activities; decentralization of urban growth; and preservation of rural communities.

The RPAA included Clarence Stein, Alexander Bing, Lewis Mumford, Charles Whitaker, Henry Wright, and Frederick Ackerman. Together, they argued that American policy makers had entirely ignored the need to plan and control urban growth. Instead, through a complex system of zoning ordinances they had adopted policies that minimized public expense and maximized speculative profits. Short-term gains had been achieved at the cost of long-term social disorder. Alone, marketplace considerations spelled disaster. Unregulated, high-priced urban land resulted in almost unbelievable population and architectural density. Real estate developers built ever-larger office buildings and apartments as they sought to exploit every available open space. Thus, American cities had become undifferentiated masses of skyscrapers, high-rise apartments, tenements, thoroughfares, and parking garages. There seemed no place for such a basic human need as discrete neighborhoods with their own parks, gardens, shops, and schools.

Profit making had taken a similar toll on the rural communities that adjoined major metropolitan areas. Rural land attracted housing developers eager to provide inexpensive housing for those who worked in the city but could not afford city rents, and rural communities were displaced by sprawling suburban developments that effectively removed their own residents from the amenities of urban life. The legacies of unplanned urbanization were urban tenements, suburban sprawl, and rural dislocation. Only by replacing real estate speculation with rational planning, declared the RPAA, could American cities fulfill their proper function as centers of human creativity and cultural enrichment while rural areas were preserved as tranquil sanctuaries for repose and renewal.

Tragically, Radburn failed to match the lofty rhetoric of the RPAA. Financial limitations and unexpected national economic collapse forced the

City Housing Corporation to scale down its ambitious plans to create a true garden city. Radburn provided no employment opportunities for its residents, only housing. Moreover, the high cost of construction restricted the community to relatively affluent junior executives and professionals. In social composition, architectural style, layout, and community activities Radburn anticipated the best of today's suburban "communities." It was attractive, safe, and thoroughly livable, particularly for families with young children. After World War II Radburn became a model suburban development as it broke with previous developments in its departure from the rigid street grid system, in the separation of automobile from pedestrian traffic, promotion of community activities, and involvement of residents in policy making. Even so, Radburn fell victim to the depression. In 1933 the City Housing Corporation declared bankruptcy. Radburn continued as a first-rate suburban development, but the RPAA dissolved itself, bringing to a close one of the most interesting chapters in American urban planning.

Daniel Schaffer in *Garden Cities: The Radburn Experience* has written a solid history, not only of Radburn but of the Regional Planning Association as well. He has placed Radburn within the larger garden city movement and pointed out its divergence from conventional urban planning. Whether dealing with complex mortgage agreements or the organizational details of the Regional Planning Association, Schaffer writes with clarity and authority. *Garden Cities* is both an introduction to twentieth-century urban planning and a solid monograph on the Regional Planning Association. It should be read by everyone concerned with the current plight of American cities. *Garden Cities* also serves as a sober reminder that even in the 1920s, when American cities were much more venturesome and manageable, a sensitive and farsighted urban vision stood little chance against profit making, expediency, and inertia.

WILLIAM B. SCOTT
Kenyon College

RONALD D. ELLER. *Miners, Millhands, and Mountaineers: Industrialization of the Appalachian South, 1880-1930*. (Twentieth-Century America Series.) Knoxville: University of Tennessee Press. 1982. Pp. xxvi, 272. Cloth \$23.50, paper \$12.50.

Ronald D. Eller, a native of and now a professor in southern Appalachia, has written a useful book about economic change and the transformation of life in the Appalachian South in the fifty years after 1880. Since, as he correctly notes, the scholarly literature on the region has many gaps and since his is the first attempt to look at industrialization and its

impact on southern Appalachia as a whole and over such a long time, Eller has put us in his debt.

In a clear, well-organized manner, Eller describes the coming of the railroad and then industry, lumber and coal in particular, to southern Appalachia. The first was, of course, a *sine qua non* for the latter two. The lumber industry grew with amazing speed after 1900 and quickly peaked; production fell off sharply after World War I. The coal boom started after the national economy recovered from the depression of the 1890s. That boom did not immediately affect all of southern Appalachia. For instance, Harlan County coal mining did not begin until 1911. Production rose rapidly and then fell precipitously in 1929, never to return to its earlier high profits. Too often such cycles in timber and coal left shattered hopes, scarred people, and blotched landscapes.

Eller follows his story over a wide area. Southern Appalachia runs from western Virginia and southern West Virginia to eastern Kentucky and Tennessee, to western North Carolina, and into northern Georgia and northwestern Alabama. Given this breadth and the length of time he covers, some chapters are bound to be stronger than others. Eller is at his best when he describes and analyzes the emergence of the timber and coal industries and when he discusses the men who built these industries. In both instances, however, he could have pushed his analyses further. For example, were those industrializers of southern Appalachia who were Southern natives, planters-revived, new men hacking out a New South, or something else?

But, even given the scope of what Eller attempts, some of this book may unsettle readers. The title indicates far more attention to the people of southern Appalachia than is given in the book. Of those people he discusses, Eller says the most about the coal miners and the world in which they lived and worked. Several of his conclusions here, especially for the early years of mining in the Appalachian South, are questionable. One could take his evidence of high labor-turnover rates from before 1925 (and frantic efforts of coal companies to attract and keep workers) and conclude that the miners in southern Appalachia once enjoyed the benefits of a labor-thin market. Certainly, one ought to be wary of describing the miners as "individualistic, fatalistic, and present-oriented" (p. 196) people helplessly caught in the clutches of omnipotent coal operators.

Eller effectively counters a notion that he considers seriously misleading. The people of southern Appalachia before 1880, he argues, were not frozen in an unchanging world that was transformed only when the railroads and industry came. Well before 1880, there was a lively trade in manufactured goods in the region. The population in the area grew so much that some were forced onto less productive farm land. Those changes might be a

critically important place to begin a thorough study of the reasons why people in southern Appalachia poured into railroad construction camps, lumber camps, and coal mines. Later, they went to textile mills and garment plants, both of which Eller chose to slight. This rapid migration suggests that preindustrial Appalachia was not as happy a place as Eller implies.

Some of the questions readers will have about this book result from its discursive style. Eller hints at the imperial attitudes of the United States Forest Service and the National Park Service without developing the idea very much. To take another example, he points to the continuing poverty of southern Appalachia despite its rich natural resources without making a serious attempt to deal with the issue he has raised.

Still, scholars and others should be pleased to have this helpful book for their use and stimulus.

TOM E. TERRILL
University of South Carolina

DANIEL JOSEPH SINGAL. *The War Within: From Victorian to Modernist Thought in the South, 1919–1945*. (Fred W. Morrison Series in Southern Studies.) Chapel Hill: University of North Carolina Press. 1982. Pp. xvi, 453. Cloth \$27.00, paper \$12.00.

Daniel Joseph Singal studies the transition of the twentieth-century Southern mind from post-Victorian to modernist thought in part because he believes that change is clear, concentrated, and containable. The book is organized around psycho-biographical portraits and careful textual criticism and is divided into three categories: the lost cause plantation myth and the industrial New South creed of post-Victorianism; the reluctant moderns who rejected to some degree both planters and industry, but mainly were recidivists; and the full-blown moderns who have freed themselves from the burdens of Southern history. A coda suggests that the modern South has progressed no further in thought and aesthetic awareness but that "with its acute sense of loss of old certainties," its writers could become postmodern.

According to the author, the post-Victorians Ulrich B. Phillips and Ellen Glasgow reflected in their works the tension between hopes for an affluent, industrial future and myths of a simpler but more refined plantation past. Unable to resolve the dilemma in their own minds, they returned to the snug past. The "moderns by the skin of their teeth" rejected New South business ideology and turned to a regional cultural identity based on the lives of small farmers who remained close to the land. But social critics such as Howard Odum could not accept the results of their reformists efforts and soon returned to the ideals of a planter elite. In the

account of *Absalom, Absalom*, perhaps the most detailed and thoughtful analysis in the book, Singal maintains that William Faulkner once and for all destroyed the myths of the plantation antebellum South. But this modernist who made enormous strides in aesthetics through stream-of-consciousness writing could not sustain his own talents. After his creative period of 1929 to 1935, Faulkner slipped back into comfortable repetition and simplification, taking solace in the deeds and values of the lost plantation elite. The Vanderbilt agrarians, many of whom became excellent modernist literary critics, also rejected industrialism but affirmed another myth of the Old South through their identification with agrarianism.

In a surprising selection of William T. Couch, Singal remarks that the editor of the University of North Carolina Press led in the final separation from the burdens of the South's past. Couch rejected all facets of post-Victorianism and set the South free at last to examine itself realistically. It was Odum's students, Rupert Vance, Guy Johnson, and Arthur Raper who argued for a class-conscious region and blamed poverty and racism on both the plantation and the industrial South, although they faltered on racial integration. Finally, it was Robert Penn Warren, particularly in his characterization of Jack Burden in *All the King's Men*, who completely exorcised the past. In a debatable but interesting conjecture Singal theorizes that Burden has learned to live without certainty and that the past is only what the consciousness makes of it.

In such a detailed, complicated, sensitive textual analysis there are bound to be problems. The author's format, which is organized around individual writers rather than topics, leads to excessive psychological commentary, repetition, and the distortion of individual thoughts to fit his categories. Much of the book takes place on the campuses of Chapel Hill and Nashville, but that setting which alternately captured and set these writers free is almost completely neglected. There is also a disturbing tone, perhaps related to the author's reasons for studying the Southern mind's course to modernism; ultimately Singal makes the South modern through freeing it from its history and thus, perhaps, from being Southern at all.

JON L. WAKELYN
Catholic University of America

JAMES R. MCGOVERN. *Anatomy of a Lynching: The Killing of Claude Neal*. Baton Rouge: Louisiana State University Press. 1982. Pp. xii, 170.

On the evening of October 26, 1934, a group of North Florida whites crossed the state line into Alabama and seized a young black man named

Claude Neal from the Brewton County jail where he had been taken for safekeeping. Neal had been accused of raping a Jackson County, Florida, white girl, Lola Cannidy. The next day, after newspapers and radio stations had announced Neal's imminent lynching, a crowd gathered at the home of Lola Cannidy at dusk. That evening on the banks of the nearby Chattahoochee River, a smaller group of whites spent the better part of two hours torturing Claude Neal in a manner that is almost impossible to comprehend. He was beaten, stabbed, burned with a red hot iron from head to toe, half strangled, and ultimately emasculated. Through it all, Neal confounded the group with his composure (at one point, he calmly asked for a cigarette). At last, mercifully, he died.

But the barbaric ritual did not end with Claude Neal's death. His body was dragged to the Cannidy house where the girl's father fired three rounds from a pistol into the forehead of the lifeless Neal. For some time the crowd, including small children with pointed wooden sticks, continued to mutilate the body. Before dawn the murderers hanged Neal from a tree in front of the Jackson County Courthouse in Marianna, Florida. Throughout the day mobs of whites roamed the streets of Marianna burning the homes of black residents and beating every black in sight. Finally a detachment of National Guard soldiers arrived and restored peace to the little Florida town.

As an episode in the history of American race relations, the lynching of Claude Neal is so barbaric that it almost defies analysis. But James McGovern, to his credit, has succeeded in writing more than a narrative account of this bloodcurdling story; he has explored its causes and its ramifications.

Insofar as the effects of the lynching are concerned, McGovern is able to document the skillful way in which Walter White and the NAACP mobilized national public indignation behind a broad campaign against lynching and a specific effort to push through the Wagner-Costigan antilynching bill. While he is not the first historian to trace the steps by which Franklin Roosevelt avoided taking a public stand on the subject out of deference to his Southern congressional allies, McGovern's account—coming on the heels of his description of the Neal case—is depressing evidence of Roosevelt's timidity on even so clear-cut a moral issue as mob violence.

While the antilynching bill failed, McGovern argues that the Neal affair was a critical factor in mobilizing national and Southern support for firmer antilynching stands by public officials. (Although, in the South, it was sometimes less a matter of conscience than a concern over the impact such grisly episodes had on economic growth.) While white mobs continued to lynch and murder blacks in the South, the incidence of such acts declined drasti-

cally and their character as unconcealed festive occasions moderated.

Even more valuable is McGovern's effort to explain the social and cultural context in which such a nightmare could take place. He offers provocative observations on Southern child-rearing patterns and cautious parallels with the history of Nazi antisemitism and the common feeling of powerlessness that Southern blacks and European Jews shared. And he is particularly sensitive to the specific setting of his story. Marianna, Florida, as described by McGovern, was an all-too-typical Southern semirural community in which blacks were tolerated only as long as they scrupulously accepted their place of inferiority, violence was an accepted way of life, and the wholly inadequate legal and court system created a "disrespect for civil authority . . . lawlessness and violence. . ." (p. 32). In the end we may not have glib answers to the question of just why such a nightmare could take place, but there are more thoughtful observations than we have yet had on this most American form of mob violence.

DAN T. CARTER
Emory University

JAMES H. MADISON. *The History of Indiana*. Volume 5, *Indiana through Tradition and Change: A History of the Hoosier State and Its People, 1920-1945*. Indianapolis: Indiana Historical Society, 1982. Pp. xvii, 453. Cloth \$12.00, paper \$6.00.

This book illustrates the problems of multivolume state histories that are based upon a chronological march through the years. Four hundred pages of text, devoted to a twenty-five year period, may seem an opportunity to be quite expansive, but not really. Assuming that the work will commonly be used for reference rather than for general reading, there is an effort to be inclusive, brief, and to indicate sources fully in footnotes. In any case, the format follows that of three previously published volumes in the series. Taking into consideration this restraint, and a number of other disclaimers in his preface (p. v), James H. Madison succeeds in his purpose. Forty-five pages, about equally divided between the bibliographical essay and index, will serve well those who use the book as a reference. The author throws a wide net.

The problem for a reviewer or general reader is that when the book leaves state politics—a full third of the text—it becomes truly encyclopedic. Artfully done, to be sure, but a maze of proper nouns and alphabetical shorthand that cannot all be blamed on the New Deal. Generalizations do emerge that go beyond the Byzantine qualities of Indiana politics and government at all levels but they are generalizations usually peculiar to Indiana. This reminds us of why state histories are not displaced by regional

treatments. Those straight lines on the map do enclose separate societies.

Maybe the highest use of such volumes is as instruction for the poor souls dragooned into similar projects—remember that bicentennial fever and funding? No one has explained satisfactorily to me the philosophy of dividing dates, for instance. My volume, number 3 in a similar six-volume series, runs from 1873 to 1893—panic to panic, I guess. Indiana straightforwardly opted for years ending in zero or five for most volumes. But the real strategy is to lay back and find out what is going to precede your volume. Volumes 3 and 4 came out in 1965 and 1968 respectively—practically in tandem in this business. Looking for a general topic for purposes of comparison, I hit on education. Emma Lou Thornbrough, in volume 3, *Indiana in the Civil War Era, 1850–1880*, begins: “The years from 1850 to 1880 constitute one of the most important chapters in the history of education in Indiana” (p. 461). Clifton J. Phillips, in volume 4, *Indiana in Transition: The Emergence of an Industrial Commonwealth, 1880–1920*, is just a bit less assertive: “During the seventies and eighties, a period marked by what one Hoosier historian has called a ‘great awakening in education,’ a free, tax-supported system of common schools was securely established” (p. 386). Madison makes no similar claim. He deplores the low academic quality of Indiana’s numerous private normal schools and the relative mediocrity of Purdue and Indiana Universities compared to other Midwestern state universities. The common schools and high schools rate few cheers, outside of a few city systems.

Madison is not married to Indiana, 1920–45. I admire this detachment in an assigned volume that has eaten up an appreciable slice of the author’s productive years. Not many of us bring that gift to such an arrangement. We get involved—like Cho Cho San. This book can be used with confidence. Madison covered his chosen ground, a broad spectrum of topics. One who comes to it as a reference will find a succinct discussion, easily reached through the comprehensive index, handled with scholarly candor, and the author’s sources are there on the page in inclusive footnotes.

ROBERT C. NESBIT
University of Wisconsin,
Madison

FRANCISCO E. BALDERRAMA. *In Defense of La Raza: The Los Angeles Mexican Consulate and the Mexican Community, 1929 to 1936*. Tucson: University of Arizona Press. 1982. Pp. xii, 137. Cloth \$14.95, paper \$7.95.

Francisco E. Balderrama’s revised UCLA dissertation offers us interesting material toward the understanding of the role of consuls in the communities where they functioned. It describes the specific

relationship between Mexican government objectives and ideals and the Mexican-American and Mexican nationals in the Los Angeles area during the 1920s and 1930s. Based upon Mexican, United States, California, and local archival materials and printed sources, supplemented by newspapers and extensive oral interviewing of contemporary Southern California residents, this study has tapped the range of sources that seems necessary to cover the subject. The few past studies that have examined specific consular activities have examined the links of the consuls to entrepreneurs and financiers; Balderrama breaks new ground by focusing on the protection of nationals.

Balderrama focuses upon five problem areas that interested the consulate and “*la colonia*,” as the Mexican and Mexican-American community was called. He describes the interaction among the Mexican consulate, *la colonia*, and the Anglo community conflict with regard to such matters as deportation-repatriation, benevolent aid societies (in particular the Comité de Beneficencia Mexicana), school segregation, church-state conflict, and efforts to organize “*la raza*” farm workers. This latter issue gives insight into the limitations of the work. The organizational role of the Mexican consulate created unions for Mexican-Americans and other non-Mexican workers, as well as Mexican nationals. Thus, the consular work in organizing and protecting nationals led into areas that could be construed as interference in the domestic, social, economic, and political life of the host society. Balderrama does not make clear to what extent distinctions were made between organizing and protecting nationals and former nationals. This only receives passing attention in Balderrama’s study, but it is related to other cases. How have German, Soviet, Israeli, or Irish consuls interacted with their nationals or former nationals at crucial periods in the twentieth century? Thus, while Balderrama has entered an area as a pioneer, he has not necessarily clearly seen the broader ramifications of his work.

Balderrama’s monograph also draws our attention to the need for further study of the consular service. We need to know who composed the consular services of various countries at various times and what were the consuls’ functions with regard to financial, trade, and investment interests, to protection of nationals, to propagation of culture and ideology, and the consuls’ relationship with local communities on a daily basis. In addition to offering a starting place to begin consideration of a research strategy regarding the consular role in modern international relations, Balderrama’s book contributes to our understanding of Mexican–United States relations and of local Southern California history.

THOMAS SCHOONOVER
University of Southwestern Louisiana

KARAL ANN MARLING. *Wall-to-Wall America: A Cultural History of Post-Office Murals in the Great Depression*. Minneapolis: University of Minnesota Press. 1982. Pp. xiv, 348.

During the Great Depression the Treasury Department oversaw the placing of some eleven hundred murals in post offices. This effort involved more than art. Disagreements over style and content added a fresh dimension to the national-regional tensions that have shaped so much of American history. The program was a small part of the New Deal but testified to people's hopes and fears, their aspirations and views of themselves, and their sense of the usable past.

Most patrons wanted the murals to depict some aspect of local history that was dramatic yet personal. It had to be positive, point to the endurance of basic American values, and express belief in a prosperous and stable future drawn from the past, no matter how bad things were in the 1930s. "Firsts" also gave a community some claim to fame within national history. In charting this localism Karal Ann Marling makes shrewd comparisons to similar approaches and material in the contemporary WPA guidebooks, cinema, and cartoons.

Avoiding controversy while retaining artistic integrity occupied much of the administrators' time. Neither Congress nor local patrons would support anything radical. If unemployment figured in a mural it had to be with an emphasis on the value of labor and the prospect of returning prosperity. There were several flaps over the alleged appearance of the hammer and sickle. Nudity was impossible for both men and women. Even clad females sometimes provoked protest, as in a proposed beach scene for the Pacific Grove, California, post office. In that case a Washington administrator admonished an artist about a woman in a tight bathing suit who was "distressingly insistent physically" (p. 268). Shades of Henry James's "conscientious nudes" of the late nineteenth century!

People demanded and got recognizable subject matter and saw modernism as elitist, radical, and trivial because it supposedly appealed only to artists and critics. An artist's style could be stylized or streamlined, however, since these approaches were clear in architecture, home technology, and advertising. Only one mural was abstract, or "cubist," that of New London, Ohio, though it retained recognizable people and events. Apparently it was popular with the townspeople because it was unusual and individualistic without seeming to be pretentious.

Marling set out only to analyze the formal elements of these paintings. Fortunately the material led her elsewhere, and she has focused on their broad social meanings, both for creators and spectators. Her judgments are judicious and sometimes

striking, hinting of more such work that needs doing. Her research is impeccable, and she has analyzed enough regional examples to fashion a truly national view of the subject. The writing is sometimes a trifle cute, and the author is too defensive about her approach, apparently trying to ward off criticism from art historian peers. But they should yield to her approach. Art history remains mired in formalism and aestheticism, which remove art from the social setting. This book is an outstanding example of how to restore art to the larger culture and to see it as social expression for both artist and audience.

H. WAYNE MORGAN
University of Oklahoma

CHRISTOPHER J. KAUFFMAN. *Faith and Fraternalism: The History of the Knights of Columbus, 1882-1982*. New York: Harper and Row. 1982. Pp. xv, 512.

One million three hundred thousand Roman Catholic men belong to the Knights of Columbus, arguably one of the most influential lay religious organizations in modern religious history. Its political stances have been criticized, its theology branded traditionalist, and its activities widely hailed. By its size and impact alone, however, the Knights of Columbus has participated in most of the broad movements in American Catholicism in the past century, and to study its history is to gain fascinating insights into the role and activity of the laity throughout the period.

Fortunately, the Knights maintain extensive, professionally staffed archives, which were completely opened to Christopher J. Kauffman, a professional historian. This book was commissioned by the Knights of Columbus for their centenary, but it seems in every way to have been researched in complete freedom. Certainly, the author's analysis of the racial discrimination issue in the 1960s and the painful and often embarrassing transition made by the order following the Second Vatican Council is honest and direct.

Where this study suffers is not in its thoroughness nor its scholarly attention to avoiding partisanship but in its lack of interpretation. In the history of the Knights there are elements of most of the major questions that touched the life of the American Catholic church in the past century. A great deal of evidence is revealed about the evolving nature of lay leadership in the church during the period. Often at the nexus of politics and religion, the Knights provide useful insights into the patterns of Catholic development. On specific topics (for example, nativism) Kauffman reveals little that is new. This is a survey history of the Knights of Columbus, and it

does not take on the task of delving too far beyond events into interpretation. Nevertheless, religious historians will find here a complete and detailed history of the Knights of Columbus, its activities, and involvements. The sources are presented with care; they whet the appetite for more analytical studies.

NORBERT C. BROCKMAN
University of St. Michael's College
University of Toronto

MAURICE ISSERMAN. *Which Side Were You On? The American Communist Party during the Second World War*. Middletown, Conn.: Wesleyan University Press; distributed by Columbia University Press, New York. 1982. Pp. xx, 305. \$19.95.

The purpose of this book is to provide a more sophisticated understanding of the American Communist movement than is to be found in Theodore Draper's *The Roots of American Communism*. Draper and the other authors in the "Fund for the Republic" series interpreted American communism almost exclusively according to its subordination to the Soviet Union, thus rendering a realistic appraisal of the motives and actions of the American Communists themselves virtually impossible. As Maurice Isserman puts it, "The Fund for the Republic interpretation makes it hard to understand why anyone with intelligence and integrity would have remained in such a movement for more than the few days or weeks required to discover its gross inadequacies" (pp. viii-ix).

The book deals with four periods of the Communist party in American history: the Popular Front era; the era of the Nazi-Soviet Pact; the World War II years; and the immediate postwar era, covering a time frame of approximately ten years. Isserman declares his intention to focus upon those Communists who were in secondary leadership positions at the time World War II began and to explain how they attempted to establish a middle ground between their commitment to the Bolshevik revolutionary tradition and the American democratic tradition. He seeks to show that, even though the Communist party was admittedly an authoritarian organization, its members nevertheless enjoyed some flexibility within the constraints of party policy, and in spite of their alleged mindless rigidity they may be credited with certain achievements in the realms of political, cultural, and labor organizing.

Although one might question the effectiveness of the Communists' political organizing skills, it cannot be denied that they had a much greater influence in the political realm than their numbers suggest they should have had. They were a well-established mi-

nority in the Democratic parties of California and Washington, in the Minnesota Farmer-Labor party and in the American Labor party. Their influence was also apparent in the New Deal.

The Communists exhibited a high degree of skill in working through their various front organizations such as the American Youth Congress, the National Negro League, and the League of American Writers, but their greatest successes were achieved in the labor movement. They either controlled or strongly influenced several CIO unions and played an important role in the organization of the United Auto Workers. Their strength in the unions lay in office holding rather than rank-and-file support, and Communists holding key positions in unions frequently exhibited considerable flexibility in meeting the demands of changing conditions.

Isserman discusses all these matters and more in a generally successful effort to prove his thesis. But persuasive as his argument is, it must be noted that he actually devotes relatively little attention to the activities of individual Communists in secondary leadership positions, and virtually none to the rank and file. The primary focus of the book is on Earl Browder and the development of the opposition that led to his expulsion from the party in 1946. This does not, however, in any way detract from the credibility of Isserman's work. His argument is compelling, his research is sound, and his judgment is balanced. Although he is obviously sympathetic to the Communists, he is also suitably critical. All things considered, this is the best book yet produced on the American Communist party.

KENNETH E. HENDRICKSON, JR.
Midwestern State University

LEONARD MOSLEY. *Marshall: Hero for Our Times*. New York: Hearst Books. 1982. Pp. xiv, 570. \$18.50.

Many academic historians regret their inability to reach large audiences and bemoan the irrelevance of writing for each other in obscure journals like the *American Historical Review*. The world off campus is filled with men and women eager to learn about the past—and willing to pay substantial sums for history books. Ask Leonard Mosley.

Mosley's Marshall is a travesty—a travesty now available to untold thousands through the Book-of-the-Month Club. It will doubtless be read by many thousands more than will read Forrest Pogue's multivolume biography of Marshall or Robert Ferrell's study of Marshall as secretary of state. Mosley drapes himself in Pogue's scholarship and manages to stay out of trouble until he gets to the close of World War II—to the limit of Pogue's work. From there on the book is unmitigated nonsense, error piled on error—an unrecognizable account of

Marshall's mission to China and his years as secretary of state.

Academic historians frequently complain that too much is written on the origins of the Cold War, but let them take heart. Mosley is aware of none of it. An interview with General Albert Wedemeyer, a kindly old gentlemen who worked with Joe McCarthy in the 1950s and the John Birch Society in the 1960s, suffices to explain policy toward China. The accounts of the Yalta agreement, of the origins of the Truman Doctrine and the Marshall Plan, of Marshall, Truman, and the partition of Palestine have to be read to believe they could have been written. The vast literature on all of these subjects is ignored. But it would be hard to publish twenty-three books in thirty-seven years if one spent much time reading.

The formula for success doubtless requires a good agent and prominent friends who will write nonsense for jacket blurbs. But there is more to it. Bring Marshall to life by frequent reference to his sex life—or lack thereof. Tell the reader what secret Marshall's first wife told him as he climbed into bed to begin his honeymoon. And if you run out of gossip on Marshall, write about Eisenhower's sex life, about Kay Summersby's shapely legs. Keep the reader titillated at all cost. Magnify trivial relationships with those beautiful women Marshall allegedly adored: Madame Chiang Kai-shek, Queen Frederika of Greece, Lady Burghley, once of Bermuda. Now we have the human Marshall, the man about whom the public wants to read.

Gore Vidal is forever flaying professors (usually professors of English) for their petty concerns about detail, about accuracy on issues he considers minor. I would submit myself cheerfully as a victim here. Stated simply, George C. Marshall deserved better. I hope Dante has reserved a circle for biographers who bury their subjects in garbage. But, is the saleability of such garbage evidence that the academic historian has abdicated, that some of us are guilty of errors of omission? When are we, who presumably have taken the trouble to read the secondary literature, who will examine documents carefully, and be cautious in our use of oral history and interviews, going to write books that people will read? Mosely's Marshall should be read for no other reason than to appreciate why the field cannot be abandoned.

WARREN I. COHEN
Michigan State University

G. EDWARD WHITE. *Earl Warren: A Public Life*. New York: Oxford University Press. 1982. Pp. x, 429. \$25.00.

While conservative politicians demanded his impeachment, liberal academics ranked him with such

great chief justices of the past as John Marshall. Although born in the nineteenth century, he became a symbol of (and sometimes a scapegoat for) the social ferment and raging egalitarianism of the 1960s. Despite being criticized as a shallow legal thinker, he led the federal judiciary into a near-revolutionary transformation of American public law. Never has the Supreme Court had a more enigmatic leader than Earl Warren.

In this interpretive biography, G. Edward White offers an explanation of Warren's career that skillfully illuminates the many facets of a lengthy public life. White's study reveals consistency and continuity where others have found only a mass of inexplicable contradictions. As he tells the story, the Progressivism that became the ascendant force in California politics during Warren's college years placed an indelible stamp on the thinking of the future chief justice. It made him a nonpartisan patriot, eager to do battle with special interests and the powerful, but also a racist, capable of championing the removal of the Japanese-Americans from the West Coast during World War II, and a bluenose, able to support restrictions on the constitutional rights of gamblers and pornographers.

As a judge, Warren rejected the judicial restraint advocated by his colleague, Justice Felix Frankfurter, in favor of an activist jurisprudence that stressed the importance of reaching the "right" result in individual cases. For Warren, legal craftsmanship was vastly less important than implementing the ethical values that he saw embodied in the Constitution. The need to find support for his rulings in specific constitutional language was a matter of minor concern; the important thing was conforming the law to high principles. More and more this seemed to require affirmative governmental action and judicial intervention on behalf of individual civil liberties. As he grasped these realities, Warren, like many other reformers of his generation, moved beyond Progressivism to a modern form of liberalism.

That, at least, is how White explains Warren's public life. The interpretation is a persuasive one, and it is based on impressive research. White has gone well beyond the judicial opinions on which legal historians too often rely, making extensive use of manuscript correspondence, newspapers, oral history transcripts, and unpublished dissertations. Although once Warren's clerk, he has managed to avoid the biographer's temptation to become an apologist for his subject. Despite obvious sympathy for the chief justice, White nevertheless vigorously criticizes his role in certain incidents, such as the internment of the Japanese-Americans and the abortive appointment of Berkeley professor Max Radin to the California Supreme Court. His account is a balanced one, and he presents it in lively and quite readable prose.

Unfortunately, an otherwise excellent book is marred by some rather silly mistakes. One notable example is the reference (p. 149) to President Dwight Eisenhower's need to make a "recent" (rather than "recess") appointment to the Supreme Court after Chief Justice Fred Vinson's death in September 1953. A book of such generally high quality deserves more careful editing than this one appears to have received. One hopes the publisher will exercise greater care with the second edition. There should be one, for as White himself readily acknowledges, this is not the definitive biography of Earl Warren. That book cannot be written until Warren's Supreme Court papers become available to scholars in January 1985. One hopes that White will then undertake a revision of this slightly premature but nevertheless extremely impressive study. Even if he chooses not to do that, *Earl Warren: A Public Life* seems likely to serve for some time as the standard against which other biographies of the controversial chief justice will be measured.

MICHAEL R. BELKNAP
University of Georgia

CANADA

DEREK PETHICK. *The Nootka Connection: Europe and the Northwest Coast, 1790–1795*. Seattle: University of Washington Press or Douglas and McIntyre, Vancouver. 1980. Pp. 281. \$17.95.

The "connection" in this book's title seems to be the European and American vessels that frequented the Northwest coast of North America during the five years following the signing of the Nootka Convention. A short introductory chapter provides information on earlier voyages and on the Nootka Sound controversy, after which a chapter is devoted to each year from 1790 through 1795. These chapters begin with short summaries of their contents; Derek Pethick then identifies and gives a brief account of the activities of each of the vessels in the region during the specific year. The content of the final chapter is indicated by its title, "Retrospect and Prospect."

This is Pethick's tenth book dealing with the coastal area of British Columbia, and it cannot be said to make much of a contribution to knowledge of its subject, based as it largely is on Henry R. Wagner's *Spanish Explorations in the Strait of Juan de Fuca* (1933), Warren L. Cook's magisterial *Flood Tide of Empire* (1973), and the journal articles published by Frederick W. Howay between 1925 and 1942. The catalogue of ships seems reasonably complete, but some fairly important occurrences are omitted—for example, the groundings of George Vancouver's *Discovery* and its consort *Chatham* in Queen Charlotte Strait in August 1792. Yet the author

finds it necessary to include some accounts of the vessels' experiences in Hawaiian waters, a distant appendage of "the Northwest Coast"! Almost all of the notes refer to secondary works, and Pethick seems to take pleasure in detecting minor discrepancies in those on which he relies so heavily. One was surprised to read in note 63 (p. 251) that "the spelling of [Baranov's] name varies, and is not generally agreed upon"; can Pethick be unaware of the vagaries of transliteration?

The illustrations are adequate and relevant—one shows HMS *Discovery* on the rocks "in Queen Charlotte Sound," although it was obviously in the strait of the same name—but no credits are given. The provision of an adequate map would be a great convenience to the reader; those included depict Vancouver's surveys and are poorly reproduced.

ROBERT ERWIN JOHNSON
University of Alabama

LATIN AMERICA

ENRIQUE DUSSEL. *A History of the Church in Latin America: Colonialism to Liberation, 1492–1979*. Translated by ALAN NEELY. Grand Rapids, Mich.: William B. Eerdmans. 1981. Pp. xxiii, 360. \$21.95.

The terms of historical analysis are never set once and for all time. Each generation sees the past in new terms, recasting previous experience in the categories provided by its own problems and encounters. New bases for understanding the present are laid down, giving a sense of tradition and continuity to the new historical "project" underway. Enrique Dussel's *A History of the Church in Latin America* is an excellent example of the validity of this Crocean perspective on historical writing.

Dussel is a well-known member of the group of Latin American Catholic writers and activists who have created what is loosely known as Liberation Theology. Along with Gustavo Gutierrez, Segundo Galilea, Juan Luis Segundo, and a host of others, Dussel has helped reshape the way in which Latin Americans (and interested outsiders as well) understand the role of the Catholic church and of Catholicism as a religion in the Latin American experience. For this reason, his attempt to provide new bases and materials for the history of the Latin American church is itself of considerable historical interest.

About half the book is a translation of the third Spanish edition (1971), with additional chapters added dealing with the process leading up to the 1979 CELAM meetings in Puebla, Mexico. Most of the material Dussel presents is already familiar to specialists, but both the theological analysis and the historical perspective warrant brief explication.

The theological chapters in part I ("A Hermeneutical Introduction") are written in an extremely

dense and difficult style. Dussel's method for understanding theology grounds it in the life of Christian communities, which are themselves located in a linked set of contexts and dialectical "moments" of domination. This perspective is then used consistently and quite successfully to explain the development and daily life of church-related "mediations" (institutions, rites, liturgies) and their intersection with Latin American society and culture from the conquest to the present day. Insightful comments on the nature of "popular" and "folk" Catholicism and their relation to structures of domination are provided.

The historical discussion contains no surprises but is nonetheless useful as Dussel brings together a vast amount of descriptive materials and documentary sources from all over Latin America for his discussion of each period and issue. Unfortunately, the attention given to different periods is quite unequal: the colonial era (1492–1808) is dealt with in 34 pages; the so-called "neocolonial" period (1808–1962) occupies 48 pages; and the years from 1962 to 1979 ("The Church and Latin American Liberation") take up the last 120 pages of text.

Dussel's account of the contemporary period is highly detailed and insightful. Theological discourse and church practice on many levels are firmly grounded in the analysis of social, cultural, and political changes at work in different subcultures, nations, and institutions throughout Latin America. The author's extended consideration of changes in the understanding and practice of the different "ministries" in the church (bishops, priests, religious, and lay people) is particularly rich and interesting.

This is clearly a book for specialists. Its difficult and confusing style precludes a larger audience. At the time of original publication, 1971, Dussel's perspective could be seen as part of the leading edge of Liberation Theology in Latin America. Liberation Theology has been considerably refined and extended since that date, but this book remains, perhaps, a useful insight into the emergence and elaboration of that particular outlook in Latin American Catholicism.

DANIEL H. LEVIN
University of Michigan,
Ann Arbor

JACK A. LICATE. *Creation of a Mexican Landscape: Territorial Organization and Settlement in the Eastern Puebla Basin, 1520–1605*. (Research Paper, number 201.) Chicago: Department of Geography, University of Chicago. 1981. Pp. x, 143.

The thorough, often thoughtfully interpretive research that Jack A. Licate gave to the sixteenth-century establishment of the Spanish colonial system in Mexico's eastern Puebla basin is regrettably bur-

dened—even at times obscured—by his indulgent use of a jargon presumably in vogue among the less articulate of the contemporary cultural geographers. He writes, for example, that in his study he views "the cultural landscape not solely as an assemblage of material objects and behavior but also as an association of symbols and meanings which, when read and experienced by the researcher, enables him to understand and interpret the generative forces that beliefs and values have in fashioning human landscapes." Fortunately error-riddled verbiage of that sort is generally limited to the preface, the introductory chapter, and concluding summary paragraphs in the other chapters of his *Creation of a Mexican Landscape*, but it tends to obscure his purposes and to render his conclusions unsatisfactory.

The purposes he sought to serve in his study (as outlined in his preface) are: (1) to attempt "to untangle some of the strands which make up our understanding of the origins of the cultural landscapes of central Mexico," and (2) "to contribute the perspective of a Mexicanist to the conceptual development of human geography by demonstrating how political and religious ideology may play a dominant role in structuring the functional relations and settlement morphology of human landscapes."

The six chapters that he devoted to these purposes treat the preconquest sociopolitical history of the eastern Puebla basin; the ideological background and preconquest sociopolitical experiences of its Spanish conquerors; the Spanish conquest of the area and the subsequent impact of Spanish immigration and the ravages of disease, disorientation, and the like, on the Indian population there; the establishment of local governing, religious, and economic (primarily the *encomienda*, *repartimento*, and land measurement and definition systems) institutions and processes in the area by the Spanish; and the impact of the establishment of those institutions and processes (including resettlement of a number of Indian communities and groups) on the Indian population of the area.

Although his research yielded only limited data germane to the Puebla basin area (and, therefore, only a little "new" data), Licate utilized that data and a considerable body of data germane to New Spain in general to develop an effective and useful discussion of his topics.

One might quibble with some of his discussion and pont to some relatively minor errors in interpretation, but no useful purpose would be served thereby. Those interested in the topics he treats should avail themselves of his study and use it sans those portions that are given over to jargon-ridden, overinflated, interpretive commentary.

G. MICHAEL RILEY
University of Wisconsin,
Milwaukee

NORA HAMILTON. *The Limits of State Autonomy: Post-Revolutionary Mexico*. Princeton: Princeton University Press. 1982. Pp. xvii, 391. Cloth \$36.00, paper \$8.95.

An examination of the limits of state autonomy could not be a more important or timely topic for understanding Mexico's development. Nora Hamilton carefully lays out a framework for analysis and identifies significant questions relevant to this subject. Her central concern is the extent to which the state functions independently of social classes or against dominant class interests. To determine the answer to this and related questions she proposes to examine the period from 1934 to 1940 to reveal the interplay of forces limiting or permitting the growth of state autonomy. She argues that such a historical study provides us with valuable insights concerning state autonomy from 1940 to the present.

Unfortunately, although this study asks the right questions and is clearly written, it remains a parochial case study. The author's methodology is Marxist, a fact that may account for several omissions in this work. Her focus is on class interests, and these are well developed in the introductory chapters. But there is little that is surprising to the specialist here, and, more importantly, the emphasis on class analysis neglects such important questions as the impact of the state as an employer of the middle class, the influence of bureaucrats themselves as a vested interest group, and the significance of other quasi-public institutions, such as the public universities, as socializers and recruiters of public and private elites. In addition, the author does not examine significant informal social or kinship ties between political and economic elites.

Although Hamilton examines new archival data and makes some important revelations about the roles of Aaron Saénz and Manuel Gómez Morín in providing ties between economic interests and the state during the 1920s and 1930s, the reader never sees any evidence of how typical these individuals are, and their role seems distorted as part of the larger picture. By emphasizing President Cárdenas's command over governmental decisions, the author downplays or ignores the impact of other individuals. Naturally Cárdenas is the dominant figure from 1935 to 1940, but the author needs to give stronger consideration to Cárdenas's own self-interest as a motivation of decisions benefiting or limiting state autonomy. Interestingly, Hamilton concludes that progressive and conservative factions within the state were responsible for its seemingly contradictory policies toward class interests, as each fought for the upper hand. These factions, however, are never clearly identified anywhere in the book.

To test her hypotheses, the author chooses to examine the well-known decision of the Cárdenas government to nationalize petroleum. The book

provides an excellent case study of that decision, but it involves essentially international economic, not domestic, interests. Thus, the case tells us little about conflict between the state and private capital; nor are the interests of domestic and international capital, as they relate to each other and the state, clearly delineated. The conclusions are disappointing because they do not go beyond 1940. The re-emergence of populism in the early 1970s and the alliance between the private sector and the state provide experiences to test the assumptions concerning the private sector and state autonomy. Some analysis of the post-1940 era would help us to better understand the motives of the state in rescuing the ALFA industrial group and nationalizing the banks.

Hamilton is to be commended for the appendixes that provide detailed background on important financial groups. It is therefore surprising that in an otherwise excellent bibliography, she has omitted altogether many important sources (such as the two standard works on the National Action party by Mabry and Von Sauer). She has made only cursory reference to many works by political scientists in the 1970s that would have provided a fuller picture of the ties between political and economic leadership and broadened an isolated case study into a work of breadth and significance.

RODERIC A. CAMP
Central College

MILES L. WORTMAN. *Government and Society in Central America, 1680-1840*. New York: Columbia University Press. 1982. Pp. xvii, 374. \$27.50.

Miles L. Wortman undertakes the difficult task of tracing developments in six distinct regions, involving four political systems over a period of 160 years. In addition to the printed literature, he faces abundant quantities of unpublished manuscripts.

The author begins with a discussion of the late Habsburg epoch (1680-1700), viewing it positively as being loose, permissive, and resilient. Through its reward system (tax structure and pensions) and control of the church, the colonists were well served, remaining loyal to the crown. Its weakness, Wortman contends, was its failure to invest sufficient funds in the colony to prevent incursions of pirates, smugglers, and English traders. Regarding the Bourbon reformers in a negative light, the author notes that they undermined the church and destroyed local autonomy, while eroding a sense of national integration. To make matters worse, reforms were applied dilatorily owing to devastating earthquakes in the eighteenth century, along with crop failures, political squabbles, and imperial wars. Still, the Bourbons did allow more profits to remain in the colony, and there was a resurgence in

the economy, especially as many immigrants arrived from Spain, investing both capital and energy. They married into prominent creole families and formed local dynasties, the most powerful of which originated with Juan Fermín de Aycinena, the only colonist in Central America holding the title of *marqués*. First using these prosperous merchant families to strengthen the royal position, the crown later turned against them. Such treatment, along with a deteriorated economy and a weakened Spain, led to colonial flirtation with liberalism and a further rupturing of bonds to the metropolis.

The book's colonial chapters are strong in their discussions of economic conditions, whereas the last parts, treating the Independence movement and the subsequent drift to violence and separatism, are more concerned with politics. The Carrera dictatorship was something of a contradiction in terms. Wortman asserts that this reactionary administration was actually a "social revolution," seeking a return to the traditions that had maintained stability under the Habsburgs long before. Whether or not that was a desirable goal may be debated, but, in any event, it did not evolve.

Wortman's study begins the history of Central America slightly before the end of Murdo J. MacLeod's *Spanish Central America, 1520-1720*. It ends at 1840, the point at which the projected volume of R. Lee Woodward will begin. With publication of the latter we will have a modern three-volume survey that addresses the concerns of scholars of our times. Even though the work under review here somewhat neglects social history, it is nevertheless a significant addition to the literature in the field.

WILLIAM L. SHERMAN
University of Nebraska

RICHARD H. IMMERMAN. *The CIA in Guatemala: The Foreign Policy of Intervention*. (Texas Pan American Series.) Austin: University of Texas Press. 1982. Pp. x, 291. \$24.50.

This book by Richard Immerman is the most complete account to date of the downfall of Jacobo Arbenz in 1954. It is based on research in the National Archives, a variety of manuscript and oral history collections, and interviews with a number of former U.S. officials. The results of this research are especially useful for the chapters that deal directly with the planning and execution of the 1954 operation. Even if additional material is released, it is doubtful that the story will be changed too much.

In one respect Immerman parts company with some earlier accounts. He argues that although the Eisenhower administration laid the groundwork for the overthrow of the Arbenz government for almost a year, the implementation was delayed in the hope

that anti-Communist Guatemalans would oust Arbenz without any U.S. involvement. The arms shipment from Czechoslovakia, however, forced the administration to move rapidly. Immerman also joins the ranks of those historians who argue that Dwight D. Eisenhower was a strong president and in control of U.S. foreign policy.

The policies and actions of the Guatemalan government prior to 1954 produced a torrent of criticism not only from State Department officials, but also from congressmen, newspapers, and magazines. Immerman believes that all were completely mistaken in their views of Communist influence in Guatemala. He does almost nothing, however, to disprove the reports of men such as Will Lissner of *The New York Times*, except to call them "tendentious." Immerman believes that Soviet foreign policy recognized geopolitical blocs and was characterized by moderation and peaceful coexistence. Therefore, the Soviet Union was not involved in any way in Guatemala, and the handful of Guatemalan Communists were impotent and did not pose any possible threat to the United States. Unfortunately, this view ignores the extensive involvement of the Soviet Union in Latin America through local Communist parties since the 1920s, and the real network of influence that existed. Of course Immerman refuses to acknowledge (except by way of snide comment) the documentation of Communist activities by such scholars as Robert Alexander, Ronald Schneider, and Rolie Poppino.

Immerman's ideological perspective also leads him to severely distort the Guatemalan policy of the Truman administration. He argues that from 1946 on U.S. officials, consumed by anti-Communist paranoia, pursued a policy of unrelenting hostility towards the reformers in Guatemala. This one-dimensional view of the Truman period ignores about 75 percent of the evidence, and presents the men who made policy as simple-minded and semihysterical. For example, Immerman argues that the U.S. refused to provide military assistance to Guatemala in 1950 because of a fear of "Communist aggression." In fact, the rejection of the Guatemalan request was made in a long and thoughtful memorandum, and was based on the desire not to risk "... improper identification, even by implication, with any movements in Guatemala which seek United States favor by professing to be prepared to do something about the Communists there" (*Foreign Relations*, 1950, 2:928-30). In like manner the author fails to tell the reader about the efforts of U.S. officials to prevent efforts to overthrow the Guatemalan government, the threats to kill Ambassador Richard C. Patterson, and the bombing of the American embassy. Immerman also distorts U.S. policy toward the Caribbean Legion, and this could have been avoided if he had consulted the book by Charles D. Ameringer on the Democratic Left. In

short, this book gives an overly simplistic, presentist view of U.S. policy.

ROBERT FREEMAN SMITH
University of Toledo

JOHN V. LOMBARDI. *Venezuela: The Search for Order, the Dream of Progress*. (Latin American Histories.) New York: Oxford University Press. 1982. Pp. xv, 348. \$6.95.

The late James Scobie chose well in selecting John Lombardi to write the volume on Venezuela for the Oxford University Press "Latin American Histories" series. In authoritative and graceful prose, he synthesizes in four chapters the history of Venezuela from 1500 to 1980. In addition, a preliminary chapter examines the land, resources, and people of Venezuela while a final chapter surveys Venezuelan intellectual and cultural history, with primary attention to the narrative literature that most directly reflects Venezuela's national identity. In general, he strikes a balance between traditional Venezuelan historians, whose preoccupation is with the colonial and independence periods, and North American scholars, whose fascination with the recent past has caused them to neglect prepetroleum Venezuela.

Of the unifying themes the author selects to provide a conceptual framework for an understanding of Venezuelan history, two stand out. The first is the formation, beginning already in colonial days, of an urban network, which was to give Venezuela its eventual geographical shape and unique identity. In particular, he explains convincingly why and how Caracas became the obvious and necessary center of that network. The second theme is equally central to Lombardi's interpretation of Venezuelan history: the preoccupation of Venezuelan ruling elites with participating in, and deriving maximum advantage from, economic relations with the North Atlantic world. The author argues that, beginning in the sixteenth century, Venezuela has been drawn into increasingly more close and complex ties to the economies and societies of the North Atlantic. In tracing this relationship, especially since the post-World War I oil boom, he does not take a deterministic *dependentista* approach but instead emphasizes, in contrast to most dependency theorists, Venezuela's freedom to choose how it will interrelate with that world. Indeed, he asserts that this Caribbean nation's "control over its destiny is at least as comprehensive as that of England, the Scandinavian countries, Italy, or Spain" (p. 249).

The author's treatment of key historical episodes is frequently stimulating and provocative. For example, he explains the Independence movement as essentially a recognition of the need to reorganize

local elites and North Atlantic trade patterns, an interpretation that will not be accepted enthusiastically by traditional Venezuelan historians. Neither will the latter be satisfied with his thoroughly demythologized assessment of Simón Bolívar. In the post-Independence decades he sees, not patternless power struggles and predatory *caudillismo*, but rather a rational effort by competing elites, using the mechanisms and resources then available, to achieve political order and economic prosperity within the North Atlantic framework.

The "national obsession with modernity" (p. 213) is what Lombardi stresses in his treatment of post-1935 Venezuela. Thanks to undreamed-of oil revenues, Venezuela has been dramatically transformed from an underdeveloped agricultural and pastoral society to a rapidly urbanizing and industrializing society in only two generations. The author suggests, however, that Venezuelans have perhaps not yet paid the full long-term costs of their apparent prosperity. In fact, many Venezuelans today believe that Lombardi's "technocratic regimes" have already squandered unjustly and irresponsibly the proceeds of one of the greatest economic booms in the history of Latin America.

WINFIELD J. BURGGRAAFF
*University of Missouri,
Columbia*

NATHANIEL H. LEFF. *Underdevelopment and Development in Brazil*. Volume 1, *Economic Structure and Change, 1822-1947*; volume 2, *Reassessing the Obstacles to Economic Development*. Winchester, Mass.: Allen and Unwin, 1982. Pp. xvi, 251; 140. \$29.50; \$24.00.

Nathaniel H. Leff reconsiders here his articles of the late 1960s and early 1970s on the nineteenth-century Brazilian economy, in the light of the extensive literature that has since appeared. This, then, is not a work based on original research, but one of critical reassessment and synthesis. The effort is very worthwhile because the monographs he surveys have frequently talked past each other, ignoring contrary positions and failing, therefore, to advance the debate. For the most part he stands firm on positions he took in earlier articles, although they fit into an overall framework of explanation now; his weighing of the evidence persuades him, furthermore, to adopt new positions that will certainly elicit challenges and new data.

The historical problem he sets out to understand is the great delay in the initiation of economic growth. Leff's still quite tentative calculation is that per capita growth was very close to zero until 1900. The ninety years from the arrival of the Portuguese court in Rio to the second decade of the republic appear to be a lost opportunity of far-reaching

consequences: growth since then has been faster than that of the developed countries, so that present-day backwardness is still a matter of catching up.

Weaknesses at the outset included a primitive agriculture and a heavy reliance on slaves in the capital-employing sector. Expansion of output in agriculture was accompanied by higher, rather than lower prices, and landowners persuaded the government to pursue policies—monopolization of land, slavery, and large-scale immigration—that depressed wage levels throughout the period. The elastic supply of labor, Leff finds, had negative effects on income distribution, capital-labor ratios, and the introduction of new technique. Although the overall development of the economy was thereby hampered, the welfare of the upper class was enhanced. “Class interests were so disparate in this case,” Leff remarks, “as to raise serious questions concerning the validity of using the nation as the unit of analysis” (vol. 1, p. 7).

Exports, Leff believes, provided the economy with such dynamism as it exhibited. This has been “the standard process in most underdeveloped countries,” not self-sufficient growth. The principal difficulty in nineteenth-century Brazil was that the stimulus provided by exports, mainly coffee, was excessively meager.

Government developmental initiatives were extremely limited, not for lack of will, but for lack of resources. The result was an inadequate infrastructure and underutilized human resources. The government did back a certain amount of railroad building. Although these lines did not unify the national market, they provided a significant stimulus to intraregional trade, and help to explain growth after 1900. Other capital and technological inputs, however, were at low levels, not only because of depressed labor costs and the absence of corresponding government investments in overhead, but also because of alternate investment opportunities in land and slaves and high interest rates.

Leff reconciles Eulalia Lobo’s nineteenth-century price series, which shows a trend rate of more than 5 percent, with others that show much lower rates. Lobo’s series is based on the market basket of the Rio lower class, while the others were limited to imports and import-substitutable goods. Leff points out that credit and transportation bottlenecks prevented expansion of output in the domestic farm sector, provoking higher internal inflation. Inflation was also stoked by the government, which issued currency routinely as a substitute for collecting taxes, quite independently of trade or fiscal crises. Overall, nineteenth-century inflation was neutral in its effects on development.

Leff considers the concept of dependency, in the case of Brazil, as “largely a myth” (vol. 1, p. 211).

Policy makers rejected foreign influence consistently in applying protective tariffs, inflating the money supply, nationalizing railroads, and cartelizing coffee. The “imperialism of free trade” is also rejected as an explanatory scheme. The terms of trade improved during the nineteenth century, and profit remittances must have been low, otherwise foreign investment would have not been so small.

A summary can hardly do justice to Leff’s arguments, which are internally consistent and proceed from an inventive reworking of numerous sources. In places his conclusions seem excessively schematic, as when he ascribes the relative backwardness of the Northeast to comparative advantage. His failure to see any validity at all in the imperialist argument, furthermore, seems to rely partly on overlooking real events that do not fit on econometric tables. This reviewer appreciated, nonetheless, his pointing out that dependency analysis in effect assumes victims to have been masochists or fools. Leff points to the need for further studies in several areas, especially domestic agriculture. To that list one might add the growth of small manufactures, especially iron, of which Leff seems unaware. Despite these complaints, this book is recognizably the starting point from which future argument will have to proceed.

WARREN DEAN
New York University

NOBLE DAVID COOK. *The People of the Colca Valley: A Population Study*. (Dellplain Latin American Studies, number 9.) Boulder, Colo.: Westview Press. 1982. Pp. xix, 101. \$13.50.

Noble David Cook, the doyen of historical demographers specializing in the Andes, arrives at a sad but not totally unexpected conclusion at the end of this excellent little monograph: the preconquest Indian population of Collaguas was almost twice the present (1982) number. The demographic story of this tragic decline is the subject of this book. Within the context of a Great Mortality theme, Cook traces the collapse of the Indian population of the Colca River Valley (about one hundred kilometers north of Arequipa in southern Peru) and shows how major factors of disease, emigration, and ecological change resulted in disaster.

Interest in this particular microregion was sparked by the discovery in the National Archives of Lima of a 1591 government inspection report that contained fascinating information on Andean life, demography, and social and economic organization. Other region-specific data were unearthed and analyzed by a team of North American and Peruvian scholars. Tribute lists beginning in the sixteenth and seventeenth centuries, parish registers for baptisms,

marriages, and burials, and other reports provided raw data for studying trends in fertility, marriage patterns, family size, relationships between two halves of a community (moieties), and changes in economic and social structure resulting from Spanish-Indian contacts. Cook's population study is a specific part of this general research.

The author's focus is demographic. He looks at the big figures such as Viceroy Francisco de Toledo's 1570s census, Vasquez de Espinosa's reports for the early 1620s, Navarra Rocafull's count of 1681–89, and later censuses reaching the twentieth century. Cook explains what they tell us about population density and family size and what they suggest about the early population decline. From global figures Cook descends to an analysis of individual censuses of the old colonial province of Los Collaguas and even further to the records of individual parishes. When he ascends from this plethora of numbers, he has some surprising and innovative conclusions about small family size and the function and role of the *ayllu*, that smallest of Indian political and social units. It is in the careful and meticulous study of parish records that Cook traces the growth and

decline of specific sociopolitical units. One never is overwhelmed by the data. Cook asks at each important stage: what do the figures tell us about the people who lived (and still live) in Los Collaguas? The book is a model of how a historical demographer should go about his task.

The advantages of microregional studies are considerable. The research is manageable and the margin for error is reduced. The drawback comes in relating the data to macroprocesses outside of the restricted area of research. Cook has partly compensated for this through his recent, massively researched study, *Demographic Collapse: Indian Peru, 1520–1620* (1981), in which the Peruvian Andes and coastal regions are carefully scrutinized and the wider demographic problems analyzed. In *The People of the Colca Valley*, Cook not only provides us with a valuable analysis of a small Andean group, but he also lays a solid demographic foundation for further network analyses involving other social science disciplines.

NICHOLAS P. CUSHNER
Empire State College
State University of New York

Collected Essays

These volumes, recently received in the *AHR* office, do not lend themselves readily to unified reviews: the contents are therefore listed. Other similar volumes that are amenable to reviewing will be found in the review section.

KEITH ROBBINS, editor. *Religion and Humanism*. (Studies in Church History, number 17.) Oxford: Basil Blackwell, for the Ecclesiastical History Society; distributed by Biblio Distribution Center, Totowa, N.J. 1981. Pp. xii, 365. \$36.00.

DENYS HAY, Humanists, Scholars, and Religion in the Later Middle Ages. GEORGE D. S. HENDERSON, Narrative Illustration and Theological Exposition in Medieval Art. VICTORIA TUDOR, Reginald of Durham and St. Godvie of Finchale: Learning and Religion on a Personal Level. DAVID D'AVRAY, Another Friar and Antiquity. DEREK BAKER, "Arabick to the People." ROSALIND M. T. HILL, A Soldier's Devotions. DIANA M. WEBB, The Truth about Constantine: History, Hagiography, and Confusion. PETER DENLEY, Giovanni Dominici's Opposition to Humanism. ANTHONY GOODMAN, Henry VII and Christian Renewal. J. B. TRAPP, John Colet and the *Hierarchies* of the Pseudo-Dionysius. PHILIP M. J. MCNAIR, The Reformation of the Sixteenth Century in Renaissance Italy. GEORGE YULE, Medieval Piety, Humanism, and the Theology of Luther. JOHN DURKAN, Giovanni Ferrerio and Religious Humanism in Sixteenth-Century Scotland. BRUNO NEVEU, L'érudition ecclésiastique du dix-septième siècle et la nostalgie de l'antiquité chrétienne. W. B. PATTERSON, Educating the Greeks: Anglican Scholarships for Greek Orthodox Students in the Early Seventeenth Century. MICHAEL J. WALSH, The Publishing Policy of the English College Press at St. Omer, 1608–1759. EAMON DUFFY, Valentine Greatrakes, the Irish Stroker: Miracle, Science, and Orthodoxy in Restoration England. W. R. WARD, Orthodoxy, Enlightenment, and Religious Revival. ANN LOADES, Immanuel Kant's Humanism. DUNCAN NIMMO, Learning against Religion, Learning as Religion: Mark Pattison and the Victorian Crisis of Faith. SHERIDAN GILLEY, The Huxley-Wilberforce Debate: A Reconsideration. A. F. WALLS, "The Best Thinking of the Best Heathen": Humane Learning and the Missionary Movement.

THOMAS M. LENNON *et al.*, editors. *Problems of Cartesianism*. (McGill-Queen's Studies in the History of

Ideas, number 1.) Kingston, Canada: McGill-Queen's University Press. 1982. Pp. 253. \$29.85.

GREGOR SEBBA, Adrien Baillet and the Genesis of His *Vie de M. Des-Cartes*. RICHARD H. POPKIN, Cartesianism and Biblical Criticism. WALTER E. REX, Bayle, Jurieu, and the Politics of Philosophy: A Reply to Professor Popkin. JACQUES ROGER, The Cartesian Model and Its Role In Eighteenth-Century "Theory of the Earth." FRANÇOIS DUCHESNEAU, The Role of Hypotheses in Descartes's and Buffon's Theories of the Earth. RICHARD A. WATSON, Transubstantiation among the Cartesians. RONALD LAYMON, Transubstantiation: Test Case for Descartes's Theory of Space. ALAN GABBEY, *Philosophia Cartesiana Triumphata: Henry More, 1646–1671*.

ALAN R. H. BAKER and MARK BILLINGE, editors. *Period and Place: Research Methods in Historical Geography*. (Cambridge Studies in Historical Geography, number 1.) New York: Cambridge University Press. 1982. Pp. x, 377. \$49.50.

Y. BEN-ARIEH, Historical Geography in Israel: Retrospect and Prospect. R. A. BUTLIN, Developments in Historical Geography in Britain in the 1970s. M. BILLINGE, Reconstructing Societies in the Past: The Collective Biography of Local Communities. H. C. PRINCE, Modernization, Restoration, Preservation: Changes in Tastes for Antique Landscapes. H. JAEGER, Reconstructing Old Prussian Landscapes, with Special Reference to Spatial Organization. P. M. KOROSCIL, Soldier Settlement in British Columbia, 1915–1930: A Synchronic Analysis. D. W. MEINIG, Geographical Analysis of Imperial Expansion. J. H. GALLOWAY, Agricultural Improvement in Late-Colonial Tropical America: Sources and Issues. D. J. ROBINSON, Relating Structure to Process in Historical Population Analysis: Case Studies from Eighteenth-Century Spanish America. R. LAWTON, Questions of Scale in the Study of Population in Nineteenth-Century Britain. M. J. BOWDEN, Geographical Changes in Cities Following Disaster. D. DENECKE, Applied Historical Geography and Geographies of the Past: Historico-Geographical Change and Regional Processes in History. D. A. MCQUILLAN, The Interface of Physical and Historical Geography: The Analysis of Farming Decisions in Response to Drought Hazards on the Margins of the Great Plains. U. SPORRONG, Individualistic Features in a Communal Landscape: Some Comments on the Spatial Organization of a Rural Society. U. GÖRANSON, Land Use and Settlement Patterns in the Mälär Area of Sweden

before the Foundation of Villages. I. EGERBLADH, The Significance of Time for Rural Settlement Patterns. T. UKITA, Cotton Production in Japan before Industrialization. L. GUELKE, Historical Geography and Collingwood's Theory of Historical Knowing. R. W. CHAMBERS, Images, Acts, and Consequences: A Critical Review of Historical Geosophy. P. CLAVAL, The Image of France and Paris in Modern Times: A Historico-Geographical Problem. M. SENDA, Perceived Space in Ancient Japan. D. E. COSGROVE, Problems of Interpreting the Symbolism of Past Landscapes. A. R. H. BAKER, On Ideology and Historical Geography. D. J. GREGORY, Action and Structure in Historical Geography. W. NORTON, Historical Geography as the Evolution of Spatial Form. J. B. HARLEY, Historical Geography and Its Evidence: Reflections on Modelling Sources. C. HALL, Private Archives as Sources for Historical Geography. V. HANSEN, A Danish Land Survey from the Seventeenth Century. A. SIMMS, Cartographic Representation of Diachronic Analysis: The Example of the Origin of Towns. T. TANIOKA, A Note on Some Documentary Sources Available for Studying the Historical Geography of Japan. M. WIDGREN, Field Evidence in Historical Geography: A Negative Sample? A Source Critical Study of an Area with Fossil Forms in Östergötland, Sweden.

Scienze, credenze occulte, livelli di cultura. (Proceedings of the Covegno Internazionale di Studi, 1980.) Florence: Leo S. Olschki, for the Istituto Nazionale di Studi sul Rinascimento, Florence. 1982. Pp. vi, 557. L. 48,000.

CHARLES WEBSTER, Paracelsus and Demons: Science as a Synthesis of Popular Belief. MARCO FERRARI, Alcune vie di diffusione in Italia di idee e di testi di Paracelso. PAOLO GALLUZZI, Motivi paracelsiani nella Toscana di Cosimo II e di Don Antonio dei Medici: Alchimia, medicina "chimica" e riforma del sapere. GUIDO OLDRIANI, Sul rapporto al quotidiano in Ramo e nel ramismo. PAUL F. GRENDLER, Come Zuanne imparò a leggere: Scolari e testi in volgare nelle scuole veneziane del '500. PIERO LUCCHI, Leggere scrivere e abbaco: L'istruzione elementare agli inizi dell'età moderna. DANIELA PESCIATINI, Maestri, medici, cerusici nelle comunità rurali pisane nel XVII secolo. MARGARET SPUFFORD, The Contents of Samuel Pepys's Collection and the Bibliothèque Bleue. MARGARET PELLING, Tradition and Diversity: Medical Practice in Norwich, 1550–1640. RICHARD C. TREXLER, Aztec Priest for Christian Altars: The Theory and Practice of Reverence in New Spain. PETER BURKE, A Question of Acculturation? ADRIANO PROSPERI, "Otras Indias": Missionari della Controriforma tra contadini e salvaggi. D. P. WALKER, Demonic Possession used as Propaganda in the Later 16th Century. ELISABETH LABROUSSE, Le démon de Macon. BERNARD CAPP, The Status and Role of Astrology in Seventeenth-Century England: The Evidence of the Almanac. PAOLA ZAMBELLI, Fine del mondo o inizio della propaganda? Astrologia, filosofia della storia e propaganda politico-religiosa nel dibattito sulla congiunzione del 1524. OTTAVIA NICCOLI, Il diluvio del 1524 fra panico collettivo e irrisione carnevalesca. STEFANO CAROTTI, Comete, portentì, causalità naturale e escatologia in Filippo Melantone. JEAN CÉARD, J. A. de Chavigny: Le premier commentateur de Nostradamus. LUCIA LAZZERINI, Preistoria degli zanni: Mito e spettacolo nella coscienza popo-

lare. GIORGIO STABILE, La ruota della fortuna: Tempo ciclico e ricorso storico. GIGLIOLA FRAGNITO, Il museo di Antonio Giganti da Fossombrone. KRZYSZTOF POMIAN, Collection-microcosme et la culture de la curiosité.

GERHARD SCHULZ, editor. *Geheimdienste und Widerstandsbewegungen im Zweiten Weltkrieg.* (Sammlung Vandenhoeck.) Göttingen: Vandenhoeck und Ruprecht. 1982. Pp. 230. DM 32.

GERHARD SCHULZ, Einleitung: Alliierte Geheimdienste und Widerstandsbewegungen im Zweiten Weltkrieg. GERHARD SCHULZ, Englische Geheimdienste und europäische Widerstandsbewegungen. THOMAS KOCH, Der amerikanische Geheimdienst OSS (Office of Strategic Services) und die Widerstandsbewegungen. FRANZ KNIPPING, "Réseaux" und "Mouvements" in der französischen Résistance, 1940–1945. JÜRGEN HEIDEKING, Die "Schweizer Strassen" des europäischen Widerstands. HERMANN-JOSEF MALLMANN, Die Armia Krajowa und die alliierten Mächte: Zum polnischen Widerstand.

IAN NISH, editor. *Anglo-Japanese Alienation, 1919–1952: Papers of the Anglo-Japanese Conference on the History of the Second World War.* (International Studies.) New York: Cambridge University Press. 1982. Pp. x, 305. \$37.50.

HOSOYA CHIIRO, Britain and the United States in Japan's View of the International System, 1919–37. IAN NISH, Japan in Britain's View of the International System, 1919–37. HOSOYA CHIIRO, Britain and the United States in Japan's View of the International System, 1937–41. USUI KATSUMI, A Consideration of Anglo-Japanese Relations: Japanese Views of Britain, 1937–41. HAGIHARA NOBUTOSHI, Postscript: Anglo-Japanese Attitudes, 1940–1. PETER LOWE, Britain and the Opening of the War in Asia, 1937–41. IKEDA KIYOSHI, Japanese Strategy and the Pacific War, 1941–5. NOMURA MINORU, Military Policy-Makers behind Japanese Strategy against Britain. HENRY PROBERT, British Strategy and the Far Eastern War, 1941–5. IRIYE AKIRA, Wartime Japanese Planning for Post-War Asia. CHRISTOPHER THORNE, Wartime Japanese Planning for Post-War Far East. WATANABE AKIO, From Bitter Enmity to Cold Partnership: Japanese Views of the United Kingdom, 1945–52. GORDON DANIELS, Britain's View of Post-War Japan, 1945–52. GORDON DANIELS, Britain's View of Post-War Japan, 1945–9. HOSOYA CHIIRO, Some Reflections on the Conference from the Japanese Side. DONALD CAMERON WATT, Work Completed and Work as Yet Unborn: Some Reflections on the Conference from the British Side.

CHARLES W. KEGLEY, JR. and PAT MCGOWAN, editors. *Foreign Policy: USA/USSR.* (Sage International Yearbook of Foreign Policy Studies, number 7.) Beverly Hills: Sage. 1982. Pp. 320. Cloth \$25.00, paper \$12.50.

CHARLES W. KEGLEY, JR. and PAT MCGOWAN, Comparing the Foreign Policy Behavior of the United States and the Soviet Union. DAVIS B. BOBROW, *Uncoordinated Giants.* THOMAS

W. MILBURN, PHILIP D. STEWART, and RICHARD K. HERRMANN, Perceiving the Other's Intentions. W. LADD HOLLIST and THOMAS H. JOHNSON, Political-Economic Competition: Three Alternative Simulations. CHRISTER JÖNSSON, The Ideology of Foreign Policy. WALTER G. CLEMENS, JR., The Superpowers and the Third World: Aborted Ideals and Wasted Assets. JAMES R. BLAKER and LINDA P. BRADY, Arms Control and European Security. WILLIAM J. BISHOP and DAVID S. SORENSON, Superpower Defense Expenditures and Foreign Policy. JONATHAN WILKENFELD and MICHAEL BRECHER, Superpower Crisis Management Behavior. HARVEY STARR, "Détente" or "Two against One"? The China Factor. WILLIAM R. THOMPSON and DAVID P. RAPKING, Conflict, Inertia, and Reciprocity: Coping with the Western Bloc. ANDREW K. SEMMEL, Security Assistance: U.S. and Soviet Patterns. WILLIAM C. POTTER, Nuclear Export Policy: A Soviet-American Comparison.

LEONARD CANTOR, editor. *The English Medieval Landscape*. (The Middle Ages.) Philadelphia: University of Pennsylvania Press or Croom Helm, London. 1982. Pp. 225. \$25.00.

LEONARD CANTOR, The English Medieval Landscape. TREVOR ROWLEY, Medieval Field Systems. LEONARD CANTOR, Forests, Chases, Parks, and Warrens. MICHAEL WILLIAMS, Marshland and Waste. LEONARD CANTOR, Castles, Fortified Houses, Moated Homesteads, and Monastic Settlements. PETER BIGMORE, Villages and Towns. BRIAN PAUL HINDLE, Roads and Tracks. LEONARD CANTOR, Conclusion.

DOROTHY WHITELOCK *et al.*, editors. *Ireland in Early Mediaeval Europe: Studies in Memory of Kathleen Hughes*. New York: Cambridge University Press. 1982. Pp. x, 406. \$95.00.

CLARE STANCLIFFE, Red, White, and Blue Martyrdom. DOROTHY WHITELOCK, Bishop Egred, Pehtred, and Niall. ANN HAMLIN, *Dignatio dei dominici*: An Element in the Iconography of Irish Crosses? ISABEL HENDERSON, Pictish Art and the Book of Kells. MARJORIE O. ANDERSON, Dalriada and the Creation of the Kingdom of the Scots. MOLLY MILLER, Matriliney by Treaty: The Pictish Foundation-Legend. D. A. BINCHY, A Pre-Christian Survival in Mediaeval Irish Hagiography. MICHAEL LAPIDGE, The Cult of St. Indract at Glastonbury. DONNCHADH Ó CORRÁIN, Foreign Connections and Domestic Politics: Killaloe and the Uí Briain in Twelfth-Century Hagiography. PATRICK SIMS-WILLIAMS, The Evidence for Vernacular Irish Literary Influence on Early Mediaeval Welsh Literature. WENDY DAVIES, The Latin Charter-Tradition in Western Britain, Brittany, and Ireland in the Early Mediaeval Period. T. A. M. BISHOP, *Periphyseon*: The Descent of the Uncompleted Copy. KENNETH HARRISON, Episodes in the History of Easter Cycles in Ireland. DAVID DUMVILLE, Latin and Irish in the *Annals of Ulster*, A.D. 431–1050. PETER SAWYER, The Vikings and Ireland. EDWARD JAMES, Ireland and Western Gaul in the Merovingian Period.

DOMINICK LACAPRA and STEVEN L. KAPLAN, editors. *Modern European Intellectual History: Reappraisals and*

New Perspectives. Ithaca: Cornell University Press. 1982. Pp. 317. \$29.50.

ROGER CHARTIER, Intellectual History or Sociocultural History? The French Trajectories. DOMINICK LACAPRA, Rethinking Intellectual History and Reading Texts. MARTIN JAY, Should Intellectual History Take a Linguistic Turn? Reflections on the Habermas-Gadamer Debate. HANS HELLMER, Triangular Anxieties: The Present State of European Intellectual History. MARK POSTER, The Future According to Foucault: *The Archaeology of Knowledge* and Intellectual History. E. M. HENNING, Archaeology, Deconstruction, and Intellectual History. KEITH MICHAEL BAKER, On the Problem of the Ideological Origins of the French Revolution. PETER JELAVICH, Popular Dimensions of Modernist Elite Culture: The Case of Theater in Fin-de-Siècle Munich. DAVID JAMES FISHER, Reading Freud's *Civilization and Its Discontents*. HAYDEN WHITE, Method and Ideology in Intellectual History: The Case of Henry Adams.

W. WARREN WAGAR, editor. *The Secular Mind: Transformations of Faith in Modern Europe*. New York: Holmes and Meier. 1982. Pp. xiii, 272. \$35.00.

W. WARREN WAGAR, Introduction. RICHARD S. WESTFALL, Isaac Newton's *Theologiae Gentilis Origines Philosophicae*. DAVID SPADAFORA, Secularization in British Thought, 1730–1789: Some Landmarks. JOHN FREDERICK LOGAN, Superstition, Impiety, and an Enlightened Legal Order: The Theological Politics of the Abbé Mably. JOHN T. MILLER, JR., Private Faith and Public Religion: S. T. Coleridge's Confrontation with Secularism. FRANK M. TURNER and JEFFREY VON ARX, Victorian Ethics of Belief: A Reconsideration. PHYLLIS H. STOCK, Proudhon and *Morale Indépendante*: A Variation of French Secular Morality. DAVID PACE, Freud contra Ecclesiam: Rhetorical Structures in *The Future of an Illusion*. JOYCE A. BERKMAN, Transformations in Pacifist Consciousness in England, 1914–1939. JAMES CONNORS, "Who Dies if England Live?": Christianity and the Moral Vision of George Orwell. DOUGLAS K. WOOD, The Twentieth-Century Revolt against Time: Belief and Becoming in the Thought of Berdyaev, Eliot, Huxley, and Jung. MICHAEL W. MESSMER, In Complicity with Words: The Asymptomatic Consciousness of E. M. Cioran. W. WARREN WAGAR, World's End: Secular Eschatologies in Modern Fiction.

JAMES H. JOHNSON and COLIN G. POOLEY, editors. *The Structure of Nineteenth-Century Cities*. New York: St. Martin's Press or Croom Helm, London. 1982. Pp. 312. \$32.50.

JAMES H. JOHNSON and COLIN G. POOLEY, The Internal Structure of Nineteenth-Century British Cities—An Overview. RICHARD RODGER, Rents and Ground Rents: Housing and the Land Market in Nineteenth-Century Britain. PETER J. ASPINALL, The Internal Structure of the Housebuilding Industry in Nineteenth-Century Cities. ANTHONY SUTCLIFFE, The Growth of Public Intervention in the British Urban Environment during the Nineteenth Century: A Structural Approach. DAVID R. GREEN, Street Trading in London: A Case Study of Casual Labour, 1830–60. ROGER SCOLA, Retailing in the Nineteenth-Century Town: Some Problems and Possibilities. GARETH SHAW, The Role

of Retailing in the Urban Economy. COLIN G. POOLEY, Choice and Constraint in the Nineteenth-Century City: A Basis for Residential Differentiation. DAVID CANNADINE, Residential Differentiation in Nineteenth-Century Towns: From Shapes on the Ground to Shapes in Society. RICHARD DENNIS, Stability and Change in Urban Communities: A Geographical Perspective. MICHAEL ANDERSON, Indicators of Population Change and Stability in Nineteenth-Century Cities: Some Sceptical Comments. JAMES H. JOHNSON and COLIN G. POOLEY, The Future Study of the Nineteenth-Century British City: Some Concluding Comments.

JAMES E. CRONIN and JONATHAN SCHNEER, editors. *Social Conflict and the Political Order in Modern Britain*. New Brunswick, N.J.: Rutgers University Press. 1982. Pp. 221.

JAMES E. CRONIN and JONATHAN SCHNEER, Social History and Politics in Britain. CHARLES TILLY, Britain Creates the Social Movement. LYNN HOLLEN LEES, Strikes and the Urban Hierarchy in English Industrial Towns, 1842–1901. JOE WHITE, 1910–1914 Reconsidered. JONATHAN SCHNEER, The War, the State, and the Workplace: British Dockers during 1914–1918. JAMES E. CRONIN, Coping with Labour, 1918–1926. PETER WEILER, British Labour and the Cold War: The London Dock Strike of 1949. RICHARD PRICE, Rethinking Labour History: The Importance of Work.

JAMES WALVIN, editor. *Slavery and British Society, 1776–1846*. Baton Rouge: Louisiana State University Press. 1982. Pp. 272. \$24.95.

JAMES WALVIN, Introduction. SEYMOUR DRESCHER, Public Opinion and the Destruction of British Colonial Slavery. JAMES WALVIN, The Propaganda of Anti-Slavery. BETTY FLADELAND, "Our Cause Being One and the Same": Abolitionists and Chartism. MICHAEL CRATON, Slave Culture, Resistance, and the Achievement of Emancipation in the British West Indies, 1783–1838. DAVID GEGGUS, British Opinion and the Emergence of Haiti, 1791–1805. C. DUNCAN RICE, The Missionary Context of the British Anti-Slavery Movement. B. W. HIGMAN, Slavery and the Development of Demographic Theory in the Age of the Industrial Revolution. DAVID ELTIS, Abolitionist Perceptions of Society after Slavery.

JOHN DWYER *et al.*, editors. *New Perspectives on the Politics and Culture of Early Modern Scotland*. Edinburgh: John Donald; distributed by Humanities Press, Atlantic Highlands, N.J. 1982. Pp. vii, 329. \$31.50.

ROGER A. MASON, *Rex Stoicus*: George Buchanan, James VI, and the Scottish Polity. ARTHUR H. WILLIAMSON, Scotland, Antichrist, and the Invention of Great Britain. ALLAN I. MACINNES, Scottish Gaeldom, 1638–1651: The Vernacular Response to the Covenanting Dynamic. EDWARD M. FURGOL, The Military and Ministers as Agents of Presbyterian Imperialism in England and Ireland, 1640–1648. LESLEY M. SMITH, Sackcloth for the Sinner or Punishment for the

Crime? Church and Secular Courts in Cromwellian Scotland. HUGH OUSTON, York in Edinburgh: James VII and the Patronage of Learning in Scotland, 1679–1688. PETER JONES, The Polite Academy and the Presbyterians, 1720–1770. RICHARD B. SHER, Moderates, Managers, and Popular Politics in Mid-Eighteenth Century Edinburgh: The Drysdale "Bustle" of the 1760s. JOHN DWYER and ALEXANDER MURDOCH, Paradigms and Politics: Manners, Morals, and the Rise of Henry Dundas, 1770–1784. RICHARD TEICHGRAEBER III, Rethinking *Das Adam Smith Problem*. CRAIG BEVERIDGE, Childhood and Society in Eighteenth-Century Scotland. JOHN DWYER, The Heavenly City of the Eighteenth-Century Moderate Divines.

EVELINE CRUICKSHANKS, editor. *Ideology and Conspiracy: Aspects of Jacobitism, 1689–1759*. Edinburgh: John Donald; distributed by Humanities Press, Atlantic Highlands, N.J. 1982. Pp. xi, 231. \$31.50.

EVELINE CRUICKSHANKS, Introduction. MARK GOLDIE, The Nonjurors, Episcopacy, and the Origins of the Convocation Controversy. BRUCE LENMAN, The Scottish Episcopal Clergy and the Ideology of Jacobitism. HOWARD ERSKINE-HILL, Literature and the Jacobite Cause: Was There a Rhetoric of Jacobitism? NICHOLAS ROGERS, Riot and Popular Jacobitism in Early Hanoverian England. PAUL HOPKINS, Sham Plots and Real Plots in the 1690s. PIERRE BURGER, Spymaster to Louis XIV: A Study of the Papers of the Abbé Eusèbe Renaudot. BRUNO NEVEU, A Contribution to an Inventory of Jacobite Sources. L. B. SMITH, Spain and the Jacobites, 1715–16. EDWARD GREGG, The Jacobite Career of John, Earl of Mar. CLAUDE NORDMANN, Choiseul and the Last Jacobite Attempt of 1759.

JOHN M. MERRIMAN, editor. *French Cities in the Nineteenth Century*. New York: Holmes and Meier. 1981. Pp. 304.

JOHN M. MERRIMAN, Images of the Nineteenth-Century French City. JOHN M. MERRIMAN, Restoration Town, Bourgeois City: Changing Urban Politics in Industrializing Limoges. CHARLES TILLY, Charivaris, Repertoires, and Urban Politics. TED W. MARGADANT, Proto-Urban Development and Political Mobilization during the Second Republic. DAVID M. GORDON, Industrialization and Republican Politics: The Bourgeois of Reims and Saint-Étienne under the Second Empire. ROBERT L. HERBERT, Industry in the Changing Landscape from Daubigny to Monet. LOUISE A. TILLY, Three Faces of Capitalism: Women and Work in French Cities. DON REID, Decazeville: Company Town and Working-Class Community, 1826–1914. MICHAEL P. HANAGAN, Urbanization, Worker Settlement Patterns, and Social Protest in Nineteenth-Century France. JOAN W. SCOTT, Mayors versus Police Chiefs: Socialist Municipalities Confront the French State.

KLAUS HILDEBRAND and KARL FERDINAND WERNER, editors. *Deutschland und Frankreich, 1936–1939*. Assisted by KLAUS MANFRASS. (15. Deutsch-Französisches Historikerkolloquium des Deutschen Histor-

ischen Instituts Paris, 1979; Beihefte der Francia, number 9.) Munich: Artemis. 1981. Pp. xxix, 719.

RENÉ RÉMOND, L'image de l'Allemagne dans l'opinion publique française de mars 1936 à septembre 1939. KLAUS-JÜRGEN MÜLLER, Die deutsche öffentliche Meinung und Frankreich, 1933–1939. RITA THALMANN, L'émigration allemande et l'opinion française de 1936 à 1939. HANS HÖRLING, Das Deutschlandbild der Pariser Tagespresse vom Münchner Abkommen bis zum Ausbruch des II. Weltkrieges: Quantitative Analyse. HENRY DUTAILLY, Programmes d'armement et structures modernes dans l'Armée de Terre, 1935–1939. WILHELM DEIST, Heeresrüstung und Aggression, 1936–1939. PHILIPPE MASSON, La Marine française et la stratégie alliée, 1938–1939. GERHARD SCHREIBER, Die Rolle Frankreichs im strategischen und operativen Denken der deutschen Marine. CHARLES CHRISTIENNE, L'Armée de l'Air française de mars 1936 à septembre 1939. PATRICE BUFFOTOT, Le réarmement aérien allemand et l'approche de la guerre vus par le II^e Bureau Air Français. KLAUS A. MAIER, Der Aufbau der Luftwaffe und ihre strategisch-operative Konzeption, insbesondere gegenüber den Westmächten. MADELINE ASTORKIA, L'Aviation et la Guerre d'Espagne: La cinquième arme face aux exigences de la guerre moderne. RAYMOND POIDEVIN, Vers une relance des relations économiques franco-allemandes, 1938–1939. JEAN-LOUIS CRÉMIEUX-BRILHAC, La France en septembre 1939: De l'économie de crise à l'économie de guerre et l'échec de la mobilisation industrielle. HANS-JÜRGEN SCHRÖDER, Deutsch-französische Wirtschaftsbeziehungen, 1936–1939. JOST DÜLFFER, Aufrüstung, Kriegswirtschaft und soziale Frage im "Dritten Reich," 1936 bis 1939. CHARLES BLOCH, Les relations franco-allemandes et la politique des puissances pendant la Guerre d'Espagne. HANS-HENNING ABENDROTH, Deutschland, Frankreich, und der Spanische Bürgerkrieg, 1936–1939. GOTTFRIED NIEDHART, Deutsche Aussenpolitik im Entscheidungsjahr 1937. WOLFGANG MICHALKA, Die Aussenpolitik des Dritten Reiches vom österreichischen "Anschluss" bis zur Münchener Konferenz 1938. RENÉ GIRAULT, La Politique extérieure française de l'après-Munich, septembre 1938–avril 1939. FRANZ KNIPPING, Die deutsch-französische Erklärung vom 6. Dezember 1938. JACQUES BARIÉTY, La France et le problème de l'"Anschluss," mars 1936–mars 1938. JEAN-LOUIS CRÉMIEUX-BRILHAC, La France devant l'Allemagne et la guerre au début de septembre 1939. ANDREAS HILLGRUBER, Frankreich als Faktor der deutschen Aussenpolitik im Jahre 1939.

REINER STEINWEG, editor. *Hilfe + Handel = Frieden? Die Bundesrepublik in der Dritten Welt.* (Edition Suhrkamp, Neue Folge, number 97; Friedensanalysen, number 15.) Frankfurt a./M.: Suhrkamp. 1982. Pp. 418.

REINHARD RODE, Handel und Friede. RAINER TETZLAFF, Die Dritte-Welt-Politik der Bundesrepublik Deutschland zwischen Friedensrhetorik und Realpolitik: Eine Einführung mit politischen Empfehlungen. HELMUT BLEY, Namibia, die Bundesrepublik und der Westen: 15 Jahre Krisenverschärfung. FRIEDMANN BÜTTNER and THOMAS SCHEFFLER, Die Nahost-Politik der sozial-liberalen Koalition. JOACHIM BETZ, Kooperation statt Konflikt? Die Position der Bundesrepub-

lik auf den Nord-Süd-Konferenzen. JÜRGEN HÄUSLER and GEORGE SIMONIS, Modernisierung durch den Handel mit Entwicklungsländern—Rückwirkungen der Energiekrise auf das "Modell Deutschland." ROLF HOFMEIER, Bonner Entwicklungspolitik: Grundlinien und Rahmenbedingungen. GERHARD BIERWIRTH, "Ich entwickle—also bin ich!": Unauflösbare Widersprüche der Entwicklungshilfe am Beispiel eines bundesdeutschen Projekts. THOMAS HURTIENNE, Sozialismus und autozentrierte Entwicklung: Zur Korrektur eines entwicklungspolitischen Modells anhand der Beispiele China, Nordkorea, Albanien, und Kuba. KLAUS DORNER, Abrüstung durch "Nachrüstung"? Zu den Aussichten von Rüstungskontrollverhandlungen über die eurostrategischen Atomwaffen. CHRISTIAN BÜTTNER, Krieg zwischen Geburt und Tod: Zu Chaim F. Shatans "Die trauernde Seele des Soldaten."

GUY FITCH LYTLE and STEPHEN ORGEL, editor. *Patronage in the Renaissance.* (Folger Institute Essays.) Princeton: Princeton University Press, for the Folger Shakespeare Library. 1982. Pp. xiv, 389. Cloth \$37.50, paper \$14.50.

WERNER L. GUNDERSHEIMER, Patronage in the Renaissance: An Exploratory Approach. LINDA LEVY PECK, Court Patronage and Government Policy: The Jacobean Dilemma. ROBERT HARDING, Corruption and the Moral Boundaries of Patronage in the Renaissance. GUY FITCH LYTLE, Religion and the Lay Patron in Reformation England. GORDON KIPLING, Henry VII and the Origins of Tudor Patronage. MALCOLM SMUTS, The Political Failure of Stuart Court Patronage. JAN VAN DORSTEN, Literary Patronage in Elizabethan England: The Early Phase. ARTHUR F. MAROTTI, John Donne and the Rewards of Patronage. LEONARD TENNENHOUSE, Sir Walter Raleigh and the Literature of Clientage. STEPHEN ORGEL, The Royal Theatre and the Role of King. DAVID M. BERGERON, Women as Patrons of English Renaissance Drama. CHARLES HOPE, Artists, Patrons, and Advisers in the Italian Renaissance. H. W. JANSON, The Birth of "Artistic License": The Dissatisfied Patron in the Early Renaissance. DOUGLAS LEWIS, Patterns of Preference: Patronage of Sixteenth-Century Architects by the Venetian Patriciate.

STEPHEN FISCHER-GALATI *et al.*, editors. *Romania between East and West: Historical Essays in Memory of Constantin C. Giurescu.* (East European Monographs, number 103.) Boulder, Colo.: East European Monographs; distributed by Columbia University Press, New York. 1982. Pp. 414. \$25.00.

DINU C. GIURESCU, Constantin C. Giurescu, 1901–1977: Romanian Historian and Historian of Romania. J. CONSTANTIN DRĂGAN, Recollections of Constantin C. Giurescu. PAUL E. MICHELSON, The Master of Synthesis: Constantin C. Giurescu and the Coming of Age of Romanian Historiography, 1919–1947. CRISTIAN POPIȘTEANU, Professor Constantin C. Giurescu and the Mass Media. VIRGIL CÂNDEA, The "Dark Millenary" in the History of the Romanians as Seen by Constantin C. Giurescu. RAYMOND MCNALLY, Origins of the Slavic Narrative about the Historical Dracula. DEMETRIUS DVOICHENKO-MARKOV, Hetman

Ivan Mazepa in Romanian Literature. RADU R. FLORESCU, Social Classes and Revolutionary Ferment in Nineteenth-Century Bucharest. BRUCE C. FRYER, Balcescu as an Economic Historian and Theorist. LLOYD A. COHEN, The Jewish Question during the Period of the Romanian National Renaissance and the Unification of the Two Principalities of Moldavia and Wallachia, 1848–1866. GEORGE R. URSUL, from Political Freedom to Religious Independence: The Romanian Orthodox Church, 1877 to 1925. JAMES F. CLARKE, Some American Observers of the Russo-Turkish War. GH. BUZATU, Origins of Romania's Oil Policy. VICTORIA F. BROWN, The Adaptation of a Western Political Theory in a Peripheral State: The Case of Romanian Liberalism. PAUL CERNOVODEANU, Constantin C. Giurescu, Promoter of Romanian-American Scientific and Cultural Relations. GLENN E. TORREY, The Diplomatic Career of Charles J. Votipka in Romania, 1913–1920. GERALD BOBANGO and ION STANCIU, Romanian-Americans and the Union of 1918. STEPHEN FISCHER-GALATI, The Romanian-American Press as a Source of Information on Romania. PAUL D. QUINLAN, The United States and the Problem of Transylvania during World War II. ROBERT FORREST, Romanian-American Economic Relations, 1947–1975.

ALEXANDER USCHAKOW, editor. *Polen—Das Ende der Erneuerung? Gesellschaft, Wirtschaft und Kultur im Wandel*. (Beck'sche Schwarze Reihe, number 259.) Munich: C. H. Beck. 1982. Pp. 275.

STEFAN KISIELEWSKI, Die polnische Staatsräson heute. ALEXANDER USCHAKOW, Das Dilemma der polnischen Aussenpolitik. PETER RAINA, Die Intelligenz und die gesellschaftlichen Bewegungen. AUGUST PRADETTO, Sozialer Wandel und gesellschaftliches Bewusstsein. ANDRZEJ DRAWICZ, Vergangenheit und Gegenwart der polnischen Literatur. HELMUT WAGNER, Erbschaft und Nachlass der polnischen Kommunisten. BOHDAN A. OSADCZUK-KORAB, "Solidarność"—Glanz und Elend einer Gewerkschaftsbewegung. DIETER BINGEN, Die katholische Kirche im polnischen Sozialismus. REINHARD PETERHOFF, Wirtschaftliche Reformzwänge als Ausdruck von Widersprüchen in der polnischen Gesellschaftsordnung. HELENE PHILLIPP, Landwirtschaft und Nahrungsmittelversorgung. HANS-WERNER RAUTENBERG, Das Auslandspolen und seine Bedeutung für Polen. SIEGFRIED LAMMICH, Gesetzliche Absicherung des Reformkurses.

DONALD S. ZAGORIA, editor. *Soviet Policy in East Asia*. (Council on Foreign Relations Book.) New Haven: Yale University Press. 1982. Pp. xiii, 360. \$25.00.

DONALD S. ZAGORIA, The Strategic Environment in East Asia. JOHN J. STEPHAN, Asia in the Soviet Conception. ROBERT A. SCALAPINO, The Political Influence of the USSR in Asia. SEWERYN BIALER, The Sino-Soviet Conflict: The Soviet Dimension. FUJI KAMIYA, The Northern Territories: 130 Years of Japanese Talks with Czarist Russia and the Soviet Union. DONALD S. ZAGORIA and SHELDON W. SIMON, Soviet Policy in Southeast Asia. RALPH N. CLOUGH, The Soviet Union and the Two Koreas. EDWARD A. HEWETT and HERBERT S. LEVINE, The Soviet Union's Economic Relations in Asia. ROBERT W. CAMPBELL, Prospects for Siberian Eco-

nomics Development. PAUL F. LANGER, Soviet Military Power in Asia. RICHARD H. SOLOMON, Coalition Building or Condominium? The Soviet Presence in Asia and American Policy Alternatives.

PROSSER GIFFORD and WM. ROGER LOUIS, editors. *The Transfer of Power in Africa: Decolonization, 1940–1960*. New Haven: Yale University Press. 1982. Pp. xi, 654. \$35.00.

D. A. LOW, The Asian Mirror to Tropical Africa's Independence. WILLIAM ROGER LOUIS and RONALD ROBINSON, The United States and the Liquidation of the British Empire in Tropical Africa, 1941–1951. HOLLIS R. LYNCH, Pan-African Responses in the United States to British Colonial Rule in Africa in the 1940s. TONY SMITH, Patterns in the Transfer of Power: A Comparative Study of French and British Decolonization. JOHN D. HARGREAVES, Toward the Transfer of Power in British West Africa. YVES PERSON, French West Africa and Decolonization. ELIKIA M'BOKOLO, French Colonial Policy in Equatorial Africa in the 1940s and 1950s. HENRI BRUNDSCHWIG, The Decolonization of French Black Africa. DENNIS AUSTIN, The British Point of No Return? CRANFORD PRATT, Colonial Governments and the Transfer of Power in East Africa. GRACE S. IBINGIRA, The Impact of Ethnic Demands on British Decolonization in Africa: The Example of Uganda. JEAN STENGERS, Precipitous Decolonization: The Case of the Belgian Congo. KENNETH MAXWELL, Portugal and Africa: The Last Empire. PROSSER GIFFORD, Misconceived Dominion: The Creation and Disintegration of Federation in British Central Africa. LEONARD THOMPSON, The Parting of the Ways in South Africa. JEAN SURET-CANALE, From Colonization to Independence in French Tropical Africa: The Economic Background. DAVID FIELDHOUSE, Decolonization, Development, and Dependence: A Survey of Changing Attitudes. DAVID E. GARDINIER, Decolonization in French, Belgian, and Portuguese Africa: A Bibliographical Essay. A. H. M. KIRK-GREENE, A Historiographical Perspective on the Transfer of Power in British Colonial Africa: A Bibliographical Essay.

BRUCE E. ARLINGHAUS, editor. *Arms for Africa: Military Assistance and Foreign Policy in the Developing World*. Lexington, Mass.: D. C. Heath. 1983. Pp. xiv, 233. \$26.95.

BRUCE E. ARLINGHAUS, Linkage and Leverage in African Arms Transfers. DAVID E. ALBRIGHT, Overview of Communist Arms Transfers to Sub-Saharan Africa. EDWARD J. LAURANCE, Soviet Arms Transfers in the 1980s: Declining Influence in Sub-Saharan Africa. ROGER E. KANET, Military Relations between Eastern Europe and Africa. GEORGE T. YU, Chinese Arms Transfers to Africa. CYNTHIA A. CANNIZZO, Western Approaches to Military Assistance to Sub-Saharan Africa: An Overview. EDWARD A. KOLODZIEJ and BOKANGA LOKULUTU, Security Interests and French Arms-Transfer Policy in Sub-Saharan Africa. REGINA COWEN, West German Arms Transfers to Sub-Saharan Africa: Commercialism versus Foreign Policy. JOSEPH P. SMALDONE, U.S. Arms Transfers and Security-Assistance Programs in Africa: A Review and Policy Perspective.

BRUCE E. ARLINGHAUS, *Arms Transfers to Africa in the 1980s*.

MICHAEL O'BRIEN, editor. *All Clever Men, Who Make Their Way: Critical Discourse in the Old South*. Fayetteville: University of Arkansas Press. 1982. Pp. 456. \$35.00.

MICHAEL O'BRIEN, On the Mind of the Old South and Its Accessibility. JAMES HERVEY SMITH, Sismondi's Political Economy. JESSE BURTON HARRISON, English Civilization. HUGH SWINTON LEGARÉ, German Diaries. THOMAS RODERICK DEW, Republicanism and Literature. GEORGE FREDERICK HOLMES, Schlegel's Philosophy of History. HENRY AUGUSTINE WASHINGTON, The Social System of Virginia. JAMES WARLEY MILES, The Possibility and Nature of Theology. CHARLES E. A. GAYARRÉ, The Rise and Fall of John Law. FREDERICK ADOLPHUS PORCHER, Modern ATL. LOUISA SUSANNAH MCCORD, Emfranchisement of Woman. JOHN HOLMES BOCKOCK, Emerson on History. RICHARD HENRY NISBET, American Authorship and Nathaniel Hawthorne. BASIL LANNEAU GILDERSLEEVE, The Necessity of the Classics. JAMES HENLEY THORNWELL, Memoir of Dr. Henry.

SYLVIA M. JACOBS, editor. *Black Americans and the Missionary Movement in Africa*. (Contributions in Afro-American and African Studies, number 66.) Westport, Conn.: Greenwood Press. 1982. Pp. xii, 255. \$27.50.

SYLVIA M. JACOBS, The Historical Role of Afro-Americans in American Missionary Efforts in Africa. DONALD F. ROTH, The "Black Man's Burden": The Racial Background of Afro-American Missionaries and Africa. THOMAS C. HOWARD, The Mission: Introduction. TOM W. SHICK, Rhetoric and Reality: Colonization and Afro-American Missionaries in Early Nineteenth-Century Liberia. SANDY DWAYNE MARTIN, Black Baptists, Foreign Missions, and African Colonization, 1814-1882. MANNING MARABLE, Ambiguous Legacy: Tuskegee's "Missionary" Impulse and Africa during the Moton Administration, 1915-1935. THOMAS C. HOWARD, Black American Missionary Influence on the Origins of University Education in West Africa. WALTER L. WILLIAMS, The Missionary: Introduction. WALTER L. WILLIAMS, William Henry Sheppard, Afro-American Missionary in the Congo, 1890-1910. SYLVIA M. JACOBS, Their "Special Mission": Afro-American Women as Missionaries in the Congo, 1894-1910. CAROL A. PAGE, Colonial Reaction to AME Missionaries in South Africa, 1898-1910. LILLIE M. JOHNSON, Missionary-Government Relations: Black Americans in British and Portuguese Colonies. SYLVIA M. JACOBS, The Impact of Black American Missionaries in Africa. SYLVIA M. JACOBS, Black Americans and the Missionary Movement in Africa: A Bibliography.

RONALD L. NUMBERS, editor. *Compulsory Health Insurance: The Continuing American Debate*. (Contributions in Medical History, number 11.) Westport, Conn.: Greenwood Press. 1982. Pp. xv, 172. \$27.50.

RONALD L. NUMBERS, The Specter of Socialized Medicine: American Physicians and Compulsory Health Insurance. ARTHUR J. VISELITEAR, Compulsory Health Insurance and the Definition of Public Health. GARY LAND, American Images of British Compulsory Health Insurance. THEODORE R. MARMOR, Canada's Path, America's Choices: Lessons from the Canadian Experience with National Health Insurance. MONTE M. POEN, The Truman Legacy: Retreat to Medicare. PAUL STARR, Transformation in Defeat: The Changing Objectives of National Health Insurance, 1915-1980. ROY LUBOVE, The Right to Health Care: Ethical Imperatives vs. Enforceable Claims. WILBUR J. COHEN, A Postscript on the Future.

RAYMOND BRETON and PIERRE SAVARD, editors. *The Quebec and Acadian Diaspora in North America*. Toronto: Multicultural History Society of Ontario. 1982. Pp. xix, 199. \$8.00.

JEAN DAIGLE, The Acadians: A People in Search of a Country. DAVID M. HAYNE, Emigration and Colonization: Twin Themes in Nineteenth-Century French Canadian Literature. FRANCES H. EARLY, The Rise of and Fall of Félix Albert: Some Reflections on the Aspirations of Habitant Immigrants to Lowell, Massachusetts in the Late Nineteenth Century. GERALD L. GOLD, Language and Ethnic Identity in South Louisiana: Implications of Data from Mamou Prairie. MADELEINE GIGUÈRE, The Franco-Americans: Occupational Profiles. ROBERT F. HARNEY, Franco-Americans and Ethnic Studies: Notes on a Mill Town. CHARLES CASTONGUAY, The Decline of French as Home Language in the Quebec and Acadian Diaspora of Canada and the United States. GILBERT-L. COMEAULT, "L'Affaire Forest": Franco-Manitobans in Search of Cultural and Linguistic Duality. ANDRÉ N. LALONDE, *Le Patriote de l'Ouest* and French Settlement on the Prairies, 1910-1930. DONALD CARTWRIGHT, Spatial Patterns in Franco-Ontarian Communities. ROBERT CHOQUETTE, The Role of the Church in French Ontario. DANIELLE JUTEAU-LÉ, The Franco-Ontarian Collectivity: Material and Symbolic Dimensions of its Minority Status. GAETAN VALLIÈRES, The Franco-Ontarian Experience.

JAMES A. BOUTILIER, editor. *The RCN in Retrospect, 1910-1968*. Vancouver, Canada: University of British Columbia Press. 1982. Pp. xxx, 373. \$28.00.

JAMES A. BOUTILIER, Introduction. BARRY M. GOUGH, The Royal Navy's Legacy to the Royal Canadian Navy in the Pacific, 1880-1914. NIGEL D. BRODEUR, L. P. Brodeur and the Origins of the Royal Canadian Navy. P. WILLET BROCK, Commander E. A. E. Nixon and the Royal Naval College of Canada, 1910-22. BARRY D. HUNT, The Road to Washington: Canada and Empire Naval Defence, 1918-21. HUGH FRANCIS PULLEN, The Royal Canadian Navy between the Wars, 1922-39. RICHARD H. LEIR, "Big Ship Time": The Formative Years of RCN Officers Serving in RN Capital Ships. J. H. W. KNOX, An Engineer's Outline of RCN History: Part I. FRASER M. MCKEE, Princes Three: Canada's Use of Armed Merchant Cruisers during World War II. W. G. D. LUND, The Royal Canadian Navy's Quest for Autonomy in the North West Atlantic. MARC MILLNER, Royal

Canadian Navy Participation in the Battle of the Atlantic Crisis of 1943. PATRICK BEESLY, Operational Intelligence and the Battle of the Atlantic: The Role of the Royal Navy's Submarine Tracking Room. W. A. B. DOUGLAS and JÜRGEN ROHWER, "The Most Thankless Task" Revisited: Convoys, Escorts, and Radio Intelligence in the Western Atlantic, 1941–43. L. C. AUDETTE, The Lower Deck and the Mainguy Report of 1949. JOHN BOVEY, The Destroyers' War in Korea, 1952–53. STUART SOWARD, Canadian Naval Aviation, 1915–69. J. M. LEEMING, HMCS *Labrador* and the Canadian Arctic. MICHAEL HADLEY, The Impact of Public Policy on a Naval Reserve Division. J. H. W. KNOX, An Engineer's Outline of RCN History: Part II. A. KEITH CAMERON, The Royal Canadian Navy and the Unification Crisis.

KAREN SPALDING, editor. *Essays in the Political, Economic, and Social History of Colonial Latin America.*

(University of Delaware Latin American Studies Program, Occasional Papers and Monographs, number 3.) Newark: University of Delaware Latin American Studies Program. 1982. Pp. xx, 153.

KAREN SPALDING, Introduction. STUART B. SCHWARTZ, Colonial Brazil: The Role of the State in a Slave Social Formation. JOHN N. COATSWORTH, The Limits of Colonial Absolutism: The State in Eighteenth-Century Mexico. MURDO J. MACLEOD, The Primitive Nation State, Delegations of Functions, and Results: Some Examples from Early Colonial Central America. JOHN J. TEPASKE, The Fiscal Structure of Upper Peru and the Financing of Empire. HERBERT S. KLEIN, The State and the Labor Market in Rural Bolivia in the Colonial and Early Republican Periods. MARCELLO CARMAGNANI, Local Governments and Ethnic Governments in Caxaca. JUAN A. VILLAMARIN and JUDITH E. VILLAMARIN, The Concept of Nobility in Colonial Santa Fe de Bogotá.

Documents and Bibliographies

The following collections of documents, bibliographies, and other similar works were received by the *AHR* between November 3, 1982 and January 10, 1983. Books that will be reviewed are not usually listed, but listing does not necessarily preclude subsequent review.

GENERAL

MANNHEIM, KARL. *Structures of Thinking*. Translated by JEREMY J. SHAPIRO and SHERRY WEBER NICHOLSEN. Edited by DAVID KETTLER *et al.* (International Library of Sociology.) Boston: Routledge and Kegan Paul. 1982. Pp. 292. \$30.00.

ANCIENT

BOWDER, DIANA, editor. *Who Was Who in the Greek World, 776 B.C.–30 B.C.* (Phaidon Book.) Ithaca: Cornell University Press. 1982. Pp. 227. \$29.95.

MEDIEVAL

CHIBNALL, MARJORIE, editor. *Charters and Customs of the Abbey of Holy Trinity Caen*. (Records of Social and Economic History, new series, number 5.) New York: Oxford University Press, for the British Academy. 1982. Pp. liv, 163. \$37.50.

BRITAIN AND IRELAND

CAVE, KATHRYN, editor. *The Diary of Joseph Farington*. Volume 9, *January 1808–June 1809*; volume 10, *July 1809–December 1810*. (Studies in British Art.) New Haven: Yale University Press. 1982. Pp. 3187–3500; 3501–3840. \$95.00 the set.

ELTON, G. R., editor. *The Tudor Constitution: Documents and Commentary*. 2d ed. New York: Cambridge University Press. 1982. Pp. xvii, 511.

HILL, W. SPEED, general editor. *The Folger Library Edition of the Works of Richard Hooker*. Volume 4, *Of the Laws of Ecclesiastical Polity: Attack and Response*. Edited by JOHN E. BOOTY. Cambridge: Belknap Press of Harvard University Press. 1982. Pp. 1, 274. \$45.00.

REID, DAVID, editor. *The Party-Coloured Mind: Prose Relating to the Conflict of Church and State in Seventeenth-Century Scotland*. (Association for Scottish Literary Studies, number 11.) Edinburgh: Scottish Academic Press. 1982. Pp. x, 221. \$18.00.

ROYAL COMMISSION ON HISTORICAL MANUSCRIPTS. *Papers of British Cabinet Ministers, 1782–1900*. (Guides to Sources for British History, number 1.) London: Her Majesty's Stationery Office. 1982. Pp. ix, 75. £3.95.

ROYAL COMMISSION ON HISTORICAL MANUSCRIPTS. *The Manuscript Papers of British Scientists, 1600–1940*. (Guides to

Sources for British History, number 2.) London: Her Majesty's Stationery Office. 1982. Pp. 109. £3.95.

ROYAL COMMISSION ON HISTORICAL MANUSCRIPTS. *Guide to the Location of Collections Described in the Reports and Calendars Series, 1870–1980*. (Guides to Sources for British History, number 3.) London: Her Majesty's Stationery Office. 1982. Pp. 69. £3.95.

SMITH, SIR THOMAS. *De Republica Anglorum*. Edited by MARY DEWAR. (Cambridge Studies in the History and Theory of Politics.) New York: Cambridge University Press. 1982. Pp. vii, 162. \$39.50.

FRANCE

LUC, JEAN-NOËL, editor. *La petite enfance à l'école, XIX^e–XX^e siècles: Textes officiels relatifs aux salles d'asile, aux écoles maternelles, aux classes et sections enfantines, 1829–1981*. Paris: Economica or Institut National de Recherche Pédagogique. 1982. Pp. 390. 90 fr.

PASQUIER, ETIENNE. *Le catéchisme des Jésuites*. Edited by CLAUDE SUTTO. (Publications du Centre d'Études de la Renaissance de l'Université de Sherbrooke, number 9.) Sherbrooke: Éditions de l'Université de Sherbrooke. 1982. Pp. 510.

GERMANY, AUSTRIA, AND SWITZERLAND

FREYMOND, JACQUES *et al.*, editors. *Documents Diplomatiques Suisses, 1848–1945*. Volume 6, *1914–1918: 29 juin 1914–11 novembre 1918*. Bern: Benteli. 1981. Pp. lxxiv, 902.

HILL, LEONIDAS E., editor. *Die Weizsäcker-Papiere, 1900–1932*. Berlin: Ullstein. 1982. Pp. 711. DM 84.

NATIONAL ARCHIVES AND RECORDS SERVICE. *Records of German Armed Forces High Command (Oberkommando der Wehrmacht/OKW)*. Volume 6, *Amt Ausland/Abwehr (Office of Foreign and Counterintelligence)*. (Guides to German Records Microfilmed at Alexandria, Va., number 80.) Washington: National Archives and Records Service, General Services Administration. 1982. Pp. xxiv, 149.

SCHOELEN, GEORG, editor. *Bibliographisch-historisches Handbuch des Volksvereins für das katholische Deutschland*. Foreword by HORSTWALTER HEITZER. Source Note by WOLFGANG LÖHR. (Veröffentlichungen der Kommission für Zeitgeschichte, Reihe B: Forschungen, number 36.) Mainz: Matthias-Grünwald. 1982. Pp. 624.

SCRIVERIUS, DIETER, editor. *Demontagen im Land Nordrhein-Westfalen, 1946 bis 1951: Spezialinventar zu den im Nordrhein-Westfälischen Hauptstaatsarchiv in Düsseldorf vorhandenen Demontage-Akten*. (Veröffentlichungen der Staatlichen Archive des Landes Nordrhein-Westfalen, Reihe C: Quellen und Forschungen, number 9.) Siegburg: Respublica. 1981. Pp. xxvi, 364.

ITALY

- MECACCI, ENZO. *La biblioteca di Ludovico Petrucciani: Docente di diritto a Siena nel quattrocento*. (Quaderni di "Studi Senesi," number 50.) Milan: Giuffrè. 1981. Pp. x, 170. L. 10,000.
- MINNUCCI, GIOVANNI, editor. *Le lauree dello studio senese alla fine del secolo XV*. (Quaderni di "Studi Senesi," number 51.) Milan: Giuffrè. 1981. Pp. 122. L. 7,000.

EASTERN EUROPE

- KITROMILIDES, PASCHALIS M. and MARIOS L. EVRIVIADES, compilers. *Cyprus*. (World Bibliographical Series, number 28.) Santa Barbara: Clio. 1982. Pp. xx, 193. \$31.00.
- LEMERLE, PAUL, et al. editors. *Actes de Lavra. Volume 4, Études historiques-actes Serbes: Complements et index*. (Archives de l'Athos, number 11.) Paris: P. Lethielleux. 1982. Pp. xiii, 405.
- STEVENS, J. M. et al. *British Reports on Greece, 1943-1944*. (Documents on Modern Greek History, number 1.) Edited by LARS BAERENTZEN. Copenhagen: Museum Tusculanum. 1982. Pp. xxxviii, 214. \$8.00.

ASIA

- CLEAVES, FRANCIS WOODMAN, editor and translator. *The Secret History of the Mongols: For the First Time Done into English out of the Original Tongue and Provided with an Exegetical Commentary*. Volume 1. Cambridge: Harvard University Press. 1982. Pp. lxxv, 277. \$20.00.
- Ideals of the Samurai: Writings of Japanese Warriors*. Translated by WILLIAM SCOTT WILSON. Edited by GREGORY N. LEE. Graphic Designs by KAREN MASSAD. Reprint. Burbank, Calif.: Ohara Publications. 1982. Pp. 143. \$5.95.

UNITED STATES

- BODNAR, JOHN. *Workers' World: Kinship, Community, and Protest in an Industrial Society, 1900-1940*. (Studies in Industry and Society, number 2.) Baltimore: John Hopkins University Press. 1982. Pp. xvi, 200. \$19.50.
- BOSCO, RONALD A. and GLEN M. JOHNSON, editors. *The Journals and Miscellaneous Notebooks of Ralph Waldo Emerson*. Volume 16, 1866-1882. Cambridge: Belknap Press of Harvard University Press. 1982. Pp. xxviii, 596. \$45.00.
- BOYD, JULIAN P., editor. *The Papers of Thomas Jefferson*. Volume 20, 1 April to 4 August 1791. Princeton: Princeton University Press. 1982. Pp. xxxii, 759. \$50.00.
- COLEMAN, KENNETH and MILTON READY, editors. *Original Papers, Correspondence to the Trustees, James Oglethorpe, and Others, 1732-1735*. (Colonial Records of the State of Georgia, number 20.) Athens: University of Georgia Press. 1982. Pp. x, 520. \$25.00.
- CONWAY, JILL K. *The Female Experience in Eighteenth- and Nineteenth-Century America: A Guide to the History of American Women*. Assisted by LINDA KEALEY and JANET E. SCHULTE (Garland Reference Library of Social Science, number 35.) New York: Garland. 1982. Pp. xxiv, 290. \$40.00.
- DANIELSON, ELENA S. and CHARLES G. PALM, compilers. *Herbert Hoover: A Register of His Papers in the Hoover Institution Archives*. (Hoover Press Bibliographical Series, number 63.) Stanford: Hoover Institution Press. 1983. Pp. xix, 216. \$19.95.
- DANKY, JAMES P., editor. *Women's Periodicals and Newspapers from the Eighteenth Century to 1981: A Union List of the Holdings of Madison, Wisconsin, Libraries*. Compiled by MAUREEN E. HADY et al. (Reference Publication in Women's Studies.) Boston: G. K. Hall in association with the State Historical Society of Wisconsin, Madison. 1982. Pp. xxiv, 376.
- DEPARTMENT OF STATE. *Foreign Relations of the United States, 1951*. Volume 5, *The Near East and Africa*. Washington: Government Printing Office. 1982. Pp. xxxv, 1497.
- DUNN, RICHARD S. et al., editors. *The Papers of William Penn*. Volume 2, 1680-1684. Philadelphia: University of Pennsylvania Press. 1982. Pp. xix, 710. \$45.00.
- ENGINA, PURIFICACIÓN MEDINA and REYES SILES SATURNINO, editors. *Documentos relativos a la independencia de Norteamérica existentes en Archivos españoles*. Volume 7, *Archivo General de Indias, Sección Papeles de Cuba: Correspondencia y documentación oficial de varias autoridades de Luisiana y de las dos Floridas (Años 1778-1817)*. Madrid: Ministerio de Asuntos Exteriores. 1981. Pp. x, 491.
- ETULAIN, RICHARD W. *A Bibliographical Guide to the Study of Western American Literature*. Lincoln: University of Nebraska Press. 1982. Pp. xvii, 317. \$22.50.
- FERNÁNDEZ, MARÍA FRANCISCA REPRESA and MARÍA DEL CAMINO REPRESA FERNÁNDEZ, editors. *Documentos relativos a la independencia de Norteamérica existentes en Archivos españoles*. Volume 6, *Archivo General de Simancas, Secretaría de Estado: Francia (Años 1774-1786)*. Madrid: Ministerio de Asuntos Exteriores. 1981. Pp. ix, 535.
- FRASSANITO, WILLIAM A. *Antietam: The Photographic Legacy of America's Bloodiest Day*. Reprint. New York: Charles Scribner's Sons. 1982. Pp. 304.
- GARLOCK, JOHNATHAN, compiler. *Guide to the Local Assemblies of the Knights of Labor*. Westport, Conn.: Greenwood Press. 1982. Pp. xxvii, 682. \$65.00.
- GATEWOOD, WILLARD B., JR., editor. *Free Man of Color: The Autobiography of Willis Augustus Hodges*. Knoxville: University of Tennessee Press. 1982. Pp. lxxiii, 97. \$11.95.
- GREGG, EDITH E. W., editor. *The Letters of Ellen Tucker Emerson*. In two volumes. Foreword by GAY WILSON ALLEN. Kent, Ohio: Kent State University Press. 1982. Pp. xxvii, 702; x, 681. \$75.00 the set.
- LENDER, MARK E. and JAMES KIRBY MARTIN, editors. *Citizen Soldier: The Revolutionary War Journal of Joseph Bloomfield*. (Collections of the New Jersey Historical Society, number 18.) Newark: The Society. 1982. Pp. xvii, 160. \$16.50.
- LINK, ARTHUR S. et al., editors. *The Papers of Woodrow Wilson*. Volume 40, November 20, 1916-January 23, 1917. Princeton: Princeton University Press. 1982. Pp. xxi, 598. \$30.00.
- PAPENFUSE, EDWARD C. and JOSEPH M. COALE III. *The Hammond-Harwood House Atlas of Historical Maps of Maryland, 1608-1908*. Baltimore: Johns Hopkins University Press. 1982. Pp. viii, 127. \$37.50.
- PERRY, GLEN C. H., editor. *"Dear Bart": Washington's Views of World War II*. Foreword by EDMOND P. BARNETT. (Contributions in Military History, number 31.) Westport, Conn.: Greenwood Press. 1982. Pp. xix, 341. \$29.95.
- QUINN, DAVID B. and ALISON M. QUINN, editors. *The First Colonists: Documents on the Planting of the First English Settlements in North America, 1584-1590*. Rev. ed. Raleigh: North Carolina Department of Cultural Resources, Division of Archives and History. 1982. Pp. xvii, xxv, 199. \$5.00.
- VEGA, BERNARDO, editor. *Los Estados Unidos y Trujillo, Año 1945: Colección de Documentos del Departamento de Estado y de las Fuerzas Armadas Norteamericanas*. Santo Domingo: Fundación Cultural Dominicana. 1982. Pp. 352.
- VINING, DONALD, editor. *American Diaries of World War II*. New York: Pepys. 1982. Pp. 430. Cloth \$19.50, paper \$14.50.

CANADA

- GRANATSTEIN, J. L. and PAUL STEVENS, editors. *A Reader's Guide to Canadian History*. Volume 2, *Confederation to the*

Present. Buffalo: University of Toronto Press. 1982. Pp. xiv, 329. \$8.95.

Guide des sources de l'histoire du Canada conservées en France. Ottawa: Archives publiques Canada. Pp. xix, 157. \$13.80.

MUISE, D. A., editor. *A Reader's Guide to Canadian History*. Volume 1, *Beginnings to Confederation*. Buffalo: University of Toronto Press. 1982. Pp. xv, 253. \$7.95.

WHITELAW, MARJORY, editor. *The Dalhousie Journals*. Volume 3. Ottawa: Oberon Press. 1982. Pp. 219. \$19.95.

LATIN AMERICA

ALLENDE, ANDRES R. *Acuerdos de la Honorable Junta de Representantes, 1822*. La Plata, Argentina: Provincia de Buenos Aires Ministerion de Educacion y Cultura. 1981. Pp. 311.

Other Books Received

Books listed were received by the AHR between November 3, 1982, and January 10, 1983. Books that will be reviewed are not usually listed, but listing does not necessarily preclude subsequent review.

GENERAL

- BERTSCH, GARY K., editor. *Global Policy Studies*. (Sage Focus Editions, number 53.) Beverly Hills: Sage. 1982. Pp. 232. Cloth \$20.00, paper \$9.95.
- BEST, GEOFFREY. *Homow among Men and Nations: Transformations of an Idea*. (Joanne Goodman Lectures, 1981.) Toronto: University of Toronto Press. 1982. Pp. xiv, 108. Cloth \$13.50, paper \$7.50.
- BROMBERG, WALTER. *Psychiatry between the Wars, 1918-1945: A Recollection*. (Contributions in Medical History, number 10.) Westport, Conn.: Greenwood. 1982. Pp. xxxii, 184. \$29.95.
- CARSTEN, F. L. *The Rise of Fascism*. 2d ed. Berkeley and Los Angeles: University of California Press. 1982. Pp. 279. \$7.95.
- CATCHPOLE, BRIAN. *A Map History of the Modern World: 1890 to the Present Day*. 3d ed. London: Heinemann Educational Books. 1982. Pp. 169. \$6.50.
- COON, CARLETON S. *Racial Adaptations*. Chicago: Nelson-Hall. 1982. Pp. xii, 197. Cloth \$24.95, paper \$12.95.
- EAKIN, RICHARD M. *Great Scientists Speak Again*. Reprint. Berkeley and Los Angeles: University of California Press. 1982. Pp. viii, 120. \$12.95.
- EMME, EUGENE M., editor. *Science Fiction and Space Futures: Past and Present*. (AAS History Series, number 5.) San Diego: American Astronautical Society. 1982. Pp. viii, 270. Cloth \$35.00, paper \$25.00.
- FREY, LINDA et al., editors. *Women in Western European History: A Select Chronological, Geographical, and Topical Bibliography from Antiquity to the French Revolution*. Westport, Conn.: Greenwood. 1982. Pp. lv, 760.
- HENDERSON, NICHOLAS. *The Birth of NATO*. Boulder, Colo.: Westview. 1983. Pp. xiv, 130. \$17.50.
- INTERNATIONAL COMMITTEE OF HISTORICAL SCIENCES. *International Bibliography of Historical Demography*. Paris: La Société de Démographie Historique; distributed by International Union for the Scientific Study of Population, Rouen. 1982. Pp. xviii, 231.
- JOHNSON, CHALMERS. *Revolutionary Change*. 2d ed. Stanford: Stanford University Press. 1982. Pp. vii, 217. Cloth \$17.50, paper \$6.95.
- LAMB, H. H. *Climate, History, and the Modern World*. New York: Methuen. 1982. Pp. xix, 387. Cloth \$33.00, paper \$16.95.
- LE GOFF, JACQUES and BÉLA KÖPECZI, editors. *Objet et méthodes de l'histoire de la culture*. (Actes du Colloque Franco-Hongrois de Tihany, 1977.) Paris: Centre National de la Recherche Scientifique or Akadémiai Kiadó, Budapest. 1982. Pp. 247. \$11.00.
- MILLER, JAMES. *History and Human Existence: From Marx to Merleau-Ponty*. Reprint. Berkeley and Los Angeles: University of California Press. 1982. Pp. ix, 287. \$8.95.
- PARDO, THOMAS C. *Basic Archival Workshops: A Handbook for the Workshop Organizer*. Chicago: Society of American Archivists. 1982. Pp. 70. \$11.00.
- SAITTA, ARMANDO. *Giustiniano e Maometto*. (2000 Anni di Storia, number 3.) Rome: Laterza. 1982. Pp. viii, 559. L. 36,000.
- SANTAS, GERASIMOS XENOPHON. *Socrates' Philosophy in Plato's Early Dialogues*. (Arguments of the Philosophers.) Reprint. Boston: Routledge and Kegan Paul. 1982. Pp. xiii, 343. \$10.00.
- SHONFIELD, ANDREW. *The Use of Public Power*. Edited by ZUZANNA SHONFIELD. Foreword by JOHN HICKS. New York: Oxford University Press. 1982. Pp. xxiv, 140. \$19.95.
- STEINER, ZARA, editor. *The Times Survey of Foreign Ministries of the World*. London: Times Books. 1982. Pp. 624. \$87.50.
- SULZBERGER, C. L. *Such a Peace: The Roots and Ashes of Yalta*. New York: Continuum. 1982. Pp. xii, 170. \$14.95.
- WALKER, RALPH C. S. *Kant*. (Arguments of the Philosophers.) Reprint. Boston: Routledge and Kegan Paul. 1982. Pp. xii, 201. \$9.50.
- WALLBANK, T. WALTER et al. *Civilization Past and Present*. 5th ed. Glenview, Ill.: Scott, Foresman. 1983. Pp. 966. \$23.95.
- WILLIGAN, J. DENNIS and KATHERINE A. LYNCH, editors. *Sources and Methods of Historical Demography*. (Studies in Social Discontinuity.) New York: Academic Press. 1982. Pp. xv, 505. \$39.50.
- WILSON, MARGARET DAULER. *Descartes*. (Arguments of the Philosophers.) Reprint. Boston: Routledge and Kegan Paul. 1982. Pp. xvii, 255. \$9.95.

ANCIENT

- BARNES, JONATHAN. *The Presocratic Philosophers*. (Arguments of the Philosophers.) Rev. ed. Boston: Routledge and Kegan Paul. 1982. Pp. xxiii, 703. \$19.95.
- FIGUEIRA, THOMAS J. *Aegina: Society and Politics*. (Monographs in Classical Studies.) New York: Arno. 1981. Pp. xii, 360.
- RICHMOND, IAN. *Trajan's Army on Trajan's Column*. Rev. ed. London: British School at Rome. 1982. Pp. ix, 56. £8.50.
- STARR, CHESTER G. *A History of the Ancient World*. 3d ed. New York: Oxford University Press. 1983. Pp. xvii, 742. \$29.95.
- SZIDAT, JOACHIM. *Historischer Kommentar zu Ammianus Marcellinus Buch XX-XXI. Part 2, Die Verhandlungsphase*. (Historia, Einzelschriften, number 38.) Wiesbaden: Franz Steiner. 1981. Pp. 102. DM 32.
- WEBER, KARL-WILHELM. *Funde in Etrurien*. (Sternstunden

der Archäologie, number 11.) Göttingen: Muster-Schmidt. 1982. Pp. 254. DM 35.

MEDIEVAL

BOND, GERALD A. editor and translator. *The Poetry of William VII, Count of Poitiers, IX Duke of Aquitaine*. (Garland Library of Medieval Literature, series A, number 4.) New York: Garland. 1982. Pp. cxviii, 163. \$36.00.

FERLUGA, JADRAN *et al.*, editors. *Glossar zur frühmittelalterlichen Geschichte im Östlichen Europa: Serie A. Lateinische Namen bis 900*, Volume 2, Part 7, *Caich-Carolus (filius Caroli Magni)*. Wiesbaden: Franz Steiner. 1982. Pp. 289–348. DM 32.

PRICE, LORNA. *The Plan of St. Gall: In Brief*. Berkeley and Los Angeles: University of California Press. 1982. Pp. xii, 99. Cloth \$55.00, paper \$27.50.

BRITAIN AND IRELAND

GALLAGHER, JOHN. *The Decline, Revival, and Fall of the British Empire: The Ford Lectures and Other Essays*. Edited by ANIL SEAL. New York: Cambridge University Press. 1982. Pp. xxvii, 211. \$29.50.

MARSHALL, MADELEINE FORELL and JANET TODD. *English Congregational Hymns in the Eighteenth Century*. Lexington: University Press of Kentucky. 1982. Pp. 181. \$15.50.

OLSEN, DONALD J. *Town Planning in London: The Eighteenth and Nineteenth Centuries*. 2d ed. New Haven: Yale University Press. 1982. Pp. xxv, 245. Cloth \$45.00, paper \$14.95.

FRANCE

FEBVRE, LUCIEN. *The Problem of Unbelief in the Sixteenth Century: The Religion of Rabelais*. Translated by BEATRICE GÖTTLIEB. Cambridge: Harvard University Press. 1982. Pp. xxxii, 516. \$35.00.

POIDEVIN, RAYMOND and JACQUES MARIETY. *Frankreich und Deutschland: Die Geschichte ihrer Beziehungen, 1815–1975*. Translated from the French by JOSEF BECKER and JOHANNES HAAS-HEYE. Munich: C. H. Beck. 1982. Pp. 498.

THABAULT, ROGER. *Mon village: Ses hommes, ses routes, son école*. Foreword by D'ANDRÉ SIEGFRIED. (Collection Références, number 7.) Reprint. Paris: Presses de la Fondation Nationale des Sciences Politiques. 1982. Pp. 248. 56 fr.

NORTHERN EUROPE

JAKOBSSON, SVANTE. *Fattighushjonets värld i 1800-talets Stockholm* [The World of the Paupers in the Poorhouses in Nineteenth-Century Stockholm]. Summary in English. (Acta Universitatis Upsaliensis, Studia Historica Upsaliensis, number 126.) Uppsala: Historiska Institutionen vid Uppsala Universitet; distributed by Almqvist and Wiksell International, Stockholm. Pp. 205.

LASONEN, KARI. *Siirtolaisoppilas Ruotsin kouluysteysöä: Sosiometrinen tutkimus* [A Sociometric Study of Immigrant Pupils in the Swedish Comprehensive School]. (Jyväskylä Studies in Education, Psychology, and Social Research, number 44.) Jyväskylä: Jyväskylän Yliopisto. 1981. Pp. 269.

GERMANY, AUSTRIA, AND SWITZERLAND

ENDERLE, GEORGES. *Die Auswirkungen der Weltwirtschaftskrise der dreissiger Jahre auf die personelle Einkommens- und Vermögensverteilung: Methodische und theoretische Probleme, Ergebnisse einer Fallstudie*. (Freiburger Studien zur Verteilung von Einkommen und Vermögen, number 1.) Freiburg, Switzerland: Universitätsverlag. 1982. Pp. 302.

ERHARD, BENEDIKT. *Bauernstand und Politik: Zur Geschichte des Tiroler Bauernbundes*. (Schriftenreihe der Michael Gais-

mair Gesellschaft, number 1.) Vienna: Jugend und Volk. 1981. Pp. 300.

HOLBORN, HAJO. *A History of Modern Germany: The Reformation*. Reprint. Princeton: Princeton University Press. 1982. Pp. xvi, 374, xxi.

HOLBORN, HAJO. *A History of Modern Germany, 1648–1840*. Reprint. Princeton: Princeton University Press. 1982. Pp. xii, 531, xxvi.

HOLBORN, HAJO. *A History of Modern Germany, 1840–1945*. Reprint. Princeton: Princeton University Press. 1982. Pp. xv, 818, xxvi.

REINBURG, PEGGY KELLEY. *Arp Schnitzger, Organ Builder: Catalyst for the Centuries*. Bloomington: Indiana University Press. 1982. Pp. xviii, 168. \$15.00.

EASTERN EUROPE

OBERLING, PIERRE. *The Road to Bellapais: The Turkish Cypriot Exodus to Northern Cyprus*. (Social Science Monographs, number 125; Brooklyn College Studies on Society in Change, number 25.) Boulder, Colo.: Social Science Monographs; distributed by Columbia University Press, New York. 1982. Pp. xii, 256. \$25.00.

RAJČEVIĆ, VOJO. *Revolucionarni omladinski pokret u Hrvatskoj*. Volume 2, 1929–1941 [The Revolutionary Youth Movement in Croatia. Volume 2, 1929–41]. Zagreb: CDD. 1980. Pp. 266.

ŽARIĆ, SLOBODAN. *Revolucionarni omladinski pokret u Hrvatskoj*. Volume 3, 1941–1948 [The Revolutionary Youth Movement in Croatia. Volume 3, 1941–48]. Zagreb: CDD. 1980. Pp. 216.

SOVIET UNION

AMBURGER, ERIK. *Fremde und Einheimische in Wirtschafts- und Kulturleben des neuzeitlichen Russland: Ausgewählte Aufsätze*. (Quellen und Studien zur Geschichte des Östlichen Europa, number 17.) Edited by KLAUS ZERNACK. Wiesbaden: Franz Steiner. 1982. Pp. 326. DM 66.

BIKBULATOV, N. V. *Bashkirskaja sistema rodstva* [The Bashkir System of Kinship]. Moscow: Nauka. 1981. Pp. 121. 90 k.

EGAN, DAVID R. and MELINDA A. EGAN. *V. I. Lenin: An Annotated Bibliography of English-language Sources to 1980*. Assisted by JULIE ANNE GENTNER. Metuchen, N.J.: Scarecrow. 1982. Pp. xxxiii, 482. \$32.50.

PULLAT, R. N., editor. *Chislennost' i klassovyy sostav naseleniya Rossii i SSSR, XVI–XX vv.: Sbornik statei* [Size and Class Composition of the Population of Russia and the USSR from the Sixteenth to the Twentieth Century: A Collection of Articles]. Tallin: Estonian Academy of Sciences. 1979. Pp. 162. 1 r. 90 k.

TERTZ, ABRAM [ANDREI SINYAVSKY]. *The Trial Begins*. Translated by MAX HAYWARD. *On Socialist Realism*. Translated by GEORGE DENNIS. Foreword by CZESLAW MILOSZ. Reprint. Berkeley and Los Angeles: University of California Press. 1982. Pp. 219. \$6.95.

AFRICA

MCCULLOUGH, W. STEWART. *A Short History of Syriac Christianity to the Rise of Islam*. (Scholars Press General Series, number 4.) Chico, Calif.: Scholars Press. 1982. Pp. viii, 197. \$21.95.

SPEAR, THOMAS. *Traditions of Origin and Their Interpretation: The Mijikenda of Kenya*. (Papers in International Studies, Africa Series, number 42.) Athens: Ohio University Press. 1982. Pp. xi, 163. \$13.50.

ASIA

BRAUN, OTTO. *A Comintern Agent in China, 1932–1939*. Translated by JEANNE MOORE. Foreword by DICK WILSON.

- Stanford: Stanford University Press. 1982. Pp. xiii, 278. \$25.00.
- FEUERWERKER, Y-TSI MEI. *Ding Ling's Fiction: Ideology and Narrative in Modern Chinese Literature*. (Harvard East Asian Series, number 98.) Cambridge: Harvard University Press. 1982. Pp. xii, 196. \$20.00.
- KERKVIET, BENEDICT J. *The Huk Rebellion: A Study of Peasant Revolt in the Philippines*. Berkeley and Los Angeles: University of California Press. 1982. Pp. xvii, 305. \$9.95.
- WALEY, ARTHUR. *Three Ways of Thought in Ancient China*. Reprint. Stanford: Stanford University Press. 1982. Pp. xi, 216. \$5.95.
- UNITED STATES
- ACHENBAUM, W. ANDREW. *Shades of Gray: Old Age, American Values, and Federal Policies since 1920*. (Little, Brown Series on Gerontology.) Boston: Little, Brown. 1983. Pp. xv, 216.
- AMTER, JOSEPH A. *Vietnam Verdict: A Citizen's History*. New York: Continuum. 1982. Pp. xxi, 400. \$17.50.
- ARCENEUX, WILLIAM. *Acadian General: Alfred Mouton and the Civil War*. (U.S.L. History Series, number 5.) Lafayette: Center for Louisiana Studies, University of Southwestern Louisiana. 1981. Pp. xvi, 188.
- ATHEARN, FREDERIC J. *An Isolated Empire: A History of Northwestern Colorado*. (Cultural Resources Series, number 2.) 3d ed. Denver: Bureau of Land Management, Colorado State Office. 1982. Pp. 134.
- BADER, ROBERT SMITH. *The Great Kansas Bond Scandal*. Lawrence: University Press of Kansas. 1982. Pp. xiv, 392. \$25.00.
- BALLARD, JACK S. *Development and Employment of Fixed-Wing Gunships, 1962-1972*. (United States Air Force in Southeast Asia.) Washington: Government Printing Office, for Office of Air Force History. 1982. Pp. xiv, 326.
- BARRERA, MARIO. *Race and Class in the Southwest: A Theory of Racial Inequality*. Notre Dame: University of Notre Dame Press. 1979. Pp. x, 261. Cloth \$15.95, paper \$6.95.
- BILLS, SCOTT L., editor. *Kent State, May 4: Echoes through a Decade*. Kent, Ohio: Kent State University Press. 1982. Pp. xiv, 304. \$16.50.
- BRAESTRUP, PETER. *Big Story: How the American Press and Television Reported and Interpreted the Crisis of Tet 1968 in Vietnam and Washington*. Reprint abridged ed. New Haven: Yale University Press. 1983. Pp. xviii, 613. Cloth \$25.00, paper \$9.95.
- BUCKINGHAM, WILLIAM A., JR. *Operation Ranch Hand: The Air Force and Herbicides in Southeast Asia, 1961-1971*. Washington: Government Printing Office, for Office of Air Force History. 1982. Pp. xi, 253.
- BURNHAM, WALTER DEAN. *The Current Crisis in American Politics*. New York: Oxford University Press. 1982. Pp. ix, 330. \$29.95.
- CHERNY, ROBERT W. and WILLIAM ISSEL. *San Francisco: Presidio, Port, and Pacific Metropolis*. (Golden State Series.) San Francisco: Boyd Fraser. 1981. Pp. 129.
- CODIGNOLA, LUCA. *Terre D'America e Burocrazia Romana: Simon Stock, Propaganda Fide e la colonia de Lord Baltimore a Terranova, 1621-1649*. (Nordamericana, number 7.) Venice: Marsilio. 1982. Pp. 233. L. 18,000.
- COLEMAN, ELEANOR S. *Captain Gustavus Conyngham, U.S.N.: Pirate or Privateer, 1747-1819*. Washington: University Press of America. 1982. Pp. xii, 183. Cloth \$20.75, paper \$9.50.
- COOPER, HELEN A. *John Trumbull: The Hand and Spirit of a Painter*. New Haven: Yale University Art Gallery. 1982. Pp. xv, 292. Cloth \$45.00, paper \$17.50.
- CRABB, CECIL V., JR. *The Doctrines of American Foreign Policy: Their Meaning, Role, and Future*. Baton Rouge: Louisiana State University Press. 1982. Pp. xii, 446.
- DANIEL, CLETUS E. *Bitter Harvest: A History of California Farmworkers, 1870-1941*. Reprint. Berkeley and Los Angeles: University of California Press. 1982. Pp. 348. \$8.95.
- DEBO, ANGIE. *Geronimo: The Man, His Time, His Place*. (Civilization of the American Indian, number 142.) Reprint. Norman: University of Oklahoma Press. 1982. Pp. xx, 480. \$12.95.
- DUNLAP, THOMAS R. *DDT: Scientists, Citizens, and Public Policy*. Reprint. Princeton: Princeton University Press. 1982. Pp. 318. \$9.95.
- ECKERT, ALLAN W. *Gateway to Empire*. (Winning of America Series.) Boston: Little, Brown. 1983. Pp. xiv, 688. \$20.00.
- ELLIOTT, CHARLES. "Mr. Anonymous": *Robert W. Woodruff of Coca-Cola*. Atlanta: Cherokee. 1982. Pp. 310. \$14.95.
- FOLK, EDGAR F. and BYNUM SHAW. *W. W. Holden: A Political Biography*. Winston-Salem, N.C.: John F. Blair. 1982. Pp. xi, 285. \$24.95.
- FRIENDLY, FRED W. *Minnesota Rag*. Reprint. New York: Vintage Books. 1982. Pp. 243. \$5.95.
- GEORGE, PETER. *The Emergence of Industrial America: Strategic Factors in American Economic Growth since 1870*. Albany: State University of New York Press. 1982. Pp. xii, 242.
- GLAAB, CHARLES N. and A. THEODORE BROWN. *A History of Urban America*. 3d ed. New York: Macmillan. 1983. Pp. xiv, 400. \$13.95.
- GOTTLIEB, MOSHE R. *American Anti-Nazi Resistance, 1933-1941: An Historical Analysis*. New York: KTAV. 1982. Pp. xxi, 426. \$35.00.
- GRUPE DE RECHERCHE ET D'ÉTUDES NORD-AMÉRICAINES. *Les Américains et les autres*. (Actes du GRENA, 1981.) Aix-en-Provence: Université de Provence; distributed by Jeanne Laffitte, Marseille. 1982. Pp. 185.
- HAUCK, RICHARD BOYD. *Crockett: A Bio-Bibliography*. (Popular Culture Bio-Bibliographies.) Westport, Conn.: Greenwood Press. 1982. Pp. xii, 169. \$25.00.
- HINE, ROBERT V. *In the Shadow of Frémont: Edward Kern and the Art of Exploration, 1845-1860*. 2d ed., rev. Norman: University of Oklahoma Press. 1982. Pp. xxi, 180. \$18.95.
- HIXSON, JOHN and BENJAMIN FRANKLIN COOLING. *Combined Operations in Peace and War*. Rev. ed. Carlisle Barracks, Penn.: U.S. Army Military History Institute. 1982. Pp. ix, 434.
- HORWICH, GEORGE and EDWARD J. MITCHELL, editors. *Policies for Coping with Oil-Supply Disruptions*. (AEI Symposia, number 82A.) Washington: American Enterprise Institute, for Public Policy Research. 1982. Pp. x, 188. Cloth \$15.95, paper \$7.95.
- HORWITCH, MEL. *Clipped Wings: The American SST Conflict*. Cambridge: MIT Press. 1982. Pp. x, 473. \$25.00.
- JACKSON, W. TURRENTINE. *Wells Fargo in Colorado Territory*. (Monograph Series.) Denver: Colorado Historical Society. 1982. Pp. xxvii, 84.
- JELINEK, LAWRENCE J. *Harvest Empire: A History of California Agriculture*. (Golden State Series.) 2d ed. San Francisco: Boyd and Fraser. 1982. Pp. vi, 126.
- KANAWADA, LEO V., JR. *Franklin D. Roosevelt's Diplomacy and American Catholics, Italians, and Jews*. (Studies in American History and Culture, number 37.) Ann Arbor: UMI Research Press. 1982. Pp. x, 184. \$39.95.
- KOUWENHOVEN, JOHN A. *Half a Truth is Better than None: Some Unsystematic Conjectures about Art, Disorder, and American Experience*. Chicago: University of Chicago Press. 1982. Pp. xiii, 248. \$17.95.
- KUBIAK, HIERONIM. *The Polish National Catholic Church in the United States of America from 1897 to 1980: Its Social Conditioning and Social Functions*. (Zeszyty Naukowe Uniwersytetu Jagiellońskiego, number 654; Prace Polonijne, number 6.) Warsaw: Państwowe Wydawnictwo Naukowe. 1982. Pp. 214. 180 Zi.
- LESY, MICHAEL. *Bearing Witness: A Photographic Chronicle of American Life, 1860-1945*. Preface by WARREN I. SUSMAN. New York: Pantheon. 1982. Pp. xx, 171. \$27.50.

- LEWIS, DAVID LEVERING. *When Harlem Was in Vogue*. Reprint. New York: Vintage. 1982. Pp. xiv, 381. \$7.95.
- LOFTUS, JOHN. *The Belarus Secret*. Edited by NATHAN MILLER. New York: Alfred A. Knopf. 1982. Pp. viii, 196. \$13.95.
- MAJKA, LINDA C. and THEO J. MAJKA. *Farm Workers, Agribusiness, and the State*. Philadelphia: Temple University Press. 1982. Pp. xi, 346. \$24.95.
- MCKENZIE, WILLIAM H. *Mountain to Mill: The Colorado and Wyoming Railway*. Colorado Springs: MAC. 1982. Pp. 199. \$32.95.
- MEHLS, STEVEN F. *The Valley of Opportunity: A History of West-Central Colorado*. (Cultural Resources Series, number 12.) Denver: Bureau of Land Management, Colorado State Office. 1982. Pp. 352.
- MEREDITH, ROY. *Mathew Brady's Portrait of an Era*. New York: W. W. Norton. 1982. Pp. 160. \$25.95.
- MOORE, JAMIE W. *The Lowcountry Engineers: Military Missions and Economic Development in the Charleston District, U.S. Army Corps of Engineers*. Charleston: U.S. Army Corps of Engineers. 1981. Pp. iii, 140. \$11.00.
- MORGAN, ANNE HODGES and H. WAYNE MORGAN, editors. *Oklahoma: New Views of the Forty-Sixth State*. Norman: University of Oklahoma Press. 1982. Pp. x, 308. \$16.95.
- NAST, LENORA HEILIG et al., editors. *Baltimore: A Living Renaissance*. Baltimore: Historic Baltimore Society. 1982. Pp. xiii, 320.
- NIELSON, GEORGIA PANTER. *From Sky Girl to Flight Attendant: Women and the Making of a Union*. Introduction by ALICE H. COOK. Ithaca: ILR Press. 1982. Pp. xxii, 160. Cloth \$18.50, paper \$9.95.
- ODELL, JOHN S. *U.S. International Monetary Policy: Markets, Power, and Ideas as Sources of Change*. Princeton: Princeton University Press. 1982. Pp. xvi, 385. Cloth \$35.00, paper \$8.95.
- OLIVER, ANDREW and JAMES BISHOP PEABODY, editors. *The Records of Trinity Church, Boston, 1728-1830*. (Publications of the Colonial Society of Massachusetts Collections, number 56.) Boston: The Society. 1982. Pp. ix, 523-1094.
- ORDWAY, FREDERICK L., III and MITCHELL R. SHARPE. *The Rocket Team: From the V-2 to the Saturn Moon Rocket*. Reprint. Cambridge: MIT Press. 1982. Pp. xvii, 462. \$9.95.
- PALMER, RICHARD F. and KARL D. BUTLER. *Brigham Young: The New York Years*. (Charles Redd Monographs in Western History, number 14.) Provo: Charles Redd Center for Western Studies, Brigham Young University; distributed by Signature Books. Midvale, Utah. 1982. Pp. ix, 106.
- PAYNTER, ROBERT. *Models of Spatial Inequality: Settlement Patterns in Historical Archaeology*. (Studies in Historical Archaeology.) New York: Academic Press. 1982. Pp. xiv, 302. \$28.00.
- PHIFER, EDWARD WILLIAM, JR. *Burke: The History of a North Carolina County, 1777-1920, with a Glimpse Beyond*. Rev. ed. Morganton, N.C.: The Author. 1982. Pp. xvii, 555.
- PHILLIPS, GEORGE H. *The Enduring Struggle: Indians in California History*. (Golden State Series.) San Francisco: Boyd and Fraser. 1981. Pp. 93.
- RIST, RAY C., editor. *Policy Studies: Review Annual*. Volume 6. Beverly Hills: Sage. 1982. Pp. 776. \$37.50.
- ROLLE, ANDREW. *Los Angeles: From Pueblo to City of the Future*. (Golden State Series.) San Francisco: Boyd and Fraser. 1981. Pp. 120.
- ROTH, MATTHEW. *Connecticut: An Inventory of Historic Engineering and Industrial Sites*. Assisted by BRUCE CLOUETTE and VICTOR DARNELL. Washington: Society for Industrial Archaeology. 1981. Pp. xxx, 279.
- ROUSE, PARK E., JR. *Virginia: A Pictorial History*. Reprint. New York: Charles Scribner's Sons. 1982. Pp. ix, 358. \$14.95.
- SCARPA, VINCENZA. *A Portrait of the Italians in America*. New York: Charles Scribner's Sons. 1982. Pp. xxxii, 240. \$30.00.
- SCHERER, ROBERT. *With Enough Shovels: Reagan, Bush, and Nuclear War*. New York: Random House. 1982. Pp. xv, 285. \$14.95.
- SHULIMSON, JACK. *U.S. Marines in Vietnam: An Expanding War, 1966*. (Marine Corps Vietnam Operational Histories Series.) Washington: Government Printing Office. 1982. Pp. xii, 390.
- SLOANE, ERIC. *Once upon a Time: The Way America Was*. New York: Hastings House or Saunders of Toronto, Don Mills, Ontario. 1982. Pp. 64. \$12.95.
- SMITH, MARGARET DENTON and MARY LOUISE TUCKER. *Photography in New Orleans: The Early Years, 1840-1865*. Baton Rouge: Louisiana State University Press. 1982. Pp. xiv, 194.
- STEDMAN, RAYMOND WILLIAM. *Shadows of the Indian: Stereotypes in American Culture*. Foreword by RENNARD STRICKLAND. Norman: University of Oklahoma Press. 1982. Pp. xix, 281. \$24.95.
- STEINBERG, RONNIE. *Wages and Hours: Labor and Reform in Twentieth-Century America*. (Crime, Law, and Deviance.) New Brunswick: Rutgers University Press. 1982. Pp. xxi, 274. \$25.00.
- STEVENS, JOHN D. *Shaping the First Amendment: The Development of Free Expression*. (Sage Commtext Series, number 11.) Beverly Hills: Sage. 1982. Pp. 157. Cloth \$15.00, paper \$7.95.
- TEMPLE, WAYNE C. *Stephen A. Douglas: Freemason*. Bloomington, Ill.: Masonic Book Club or Illinois Lodge of Research. 1982. Pp. xix, 109. \$9.75.
- TODD, JOHN EMERSON. *Frederick Law Olmsted*. (Twayne's World Leaders Series, number 83.) Boston: Twayne. 1982. Pp. 212. \$13.95.
- TRIMBLE, WILLIAM F. *High Frontier: A History of Aeronautics in Pennsylvania*. Pittsburgh: University of Pittsburgh Press. 1982. Pp. xiv, 344. \$12.95.
- ULIBARRI, EDUARDO. *U.S. Options in Central America*. Washington: Cuban-American National Foundation. 1982. Pp. 17. \$2.00.
- VIAHOS, MICHAEL. *America: Images of Empire*. (Occasional Papers in International Affairs.) Washington: Johns Hopkins Foreign Policy Institute, School of Advanced International Studies. 1982. Pp. 124. \$4.75.
- WEBER, FRANCIS J. *California: The Catholic Experience*. Hong Kong: Libra Press. 1981. Pp. xiv, 245. \$15.00.
- WESTBROOK, PERRY D. *The New England Town in Fact and Fiction*. East Brunswick, N.J.: Fairleigh Dickinson University Press. 1982. Pp. 286. \$30.00.
- YOUNG, JAMES HARVEY, editor. *The Early Years of Federal Food and Drug Control*. (Symposium Commemorating the 75th Anniversary of the First Food and Drugs Law of 1906; Fischelis Publication on Recent History and Trends of Pharmacy, new series, number 7.) Madison: American Institute of the History of Pharmacy with the Cooperation of the American Pharmaceutical Association. 1982. Pp. 60. \$4.90.

LATIN AMERICA

- CANADELL, CARMEL and FELIX MANITO, editors. *Els mossos d'esquadra*. Barcelona: L'Avenç. 1981. Pp. 167.
- LIEBMAN, SEYMOUR B. *New World Jewry, 1493-1825: Requiem for the Forgotten*. New York: KTAV. 1982. Pp. xv, 271. \$20.00.
- RICARD, ROBERT. *The Spiritual Conquest of Mexico: An Essay on the Apostolate and the Evangelizing Methods of the Mendicant Orders in New Spain, 1523-1572*. Reprint. Translated by LESLEY BYRD SIMPSON. Berkeley and Los Angeles: University of California Press. 1982. Pp. xii, 423. \$9.95.
- ROUQUIÉ, ALAIN. *L'état militaire en Amérique Latine*. Paris: Seuil. 1982. Pp. 475. 100 fr.
- WEST, ROBERT C., editor. *Andean Reflections: Letters from Carl O. Sauer While on a South American Trip under a Grant from the Rockefeller Foundation, 1942*. (Dellplain Latin American Studies, number 11.) Boulder, Colo.: Westview. 1982. Pp. xii, 139. \$15.00.

Communications

A communication will be considered only if it relates to an article or review published in this journal; publication is solely at the editors' discretion. Letters may not exceed seven hundred words for reviews and one thousand words for articles. They should be submitted in duplicate, typed double-spaced with wide margins, and headed "To the Editor."

TO THE EDITOR:

I was glad—at first—to see Professor Dolores Greenberg's article, "Reassessing the Power Patterns of the Industrial Revolution: An Anglo-American Comparison," in the December issue of the *Review* (87 [1982]: 1237–61). It is appropriate that the leading historical journal recognize the emergence of one of the most significant special fields in historical scholarship—technological history—which is remarkable for its interdisciplinary nature even for a discipline—history—that has long since burst its old political bonds. Unfortunately, Greenberg's article turns out to be a typical revisionist piece, complete with straw men and breathless announcements of new truths that somehow escaped some of the most distinguished historians of the past. The article begs innumerable questions, and I appreciate this opportunity to ask a few of them.

Her straw man is the proposition that, because over a century passed before steam became the dominant force in industry, historians of the industrial revolution before Greenberg (and a few others) "constructed misleading paradigms" based on the presumption of "too much for Watt's wondrous invention." This is unfair and obtuse. Since when does something have to happen instantaneously to be an efficient cause? Since when is the historian to be condemned for collapsing the time base in generalizing? Nothing could resist the power of the idea of steam when its time came. Most, if not all, great breaks with the technological past developed over what must seem, to man with his puny three-score-and-ten, like a very long time indeed. There are two great reasons for this, as any one of the outstanding

technological historians now making marks for themselves in American universities, could have told Greenberg. One is the technical development of the idea itself; the other is the development of the entire technological, political, economic, and social infrastructure that the new idea demands in order to flourish. This is the real meat of technological history, and it is hopeless to try to distinguish cause from effect, forward from backward linkages, or any of the other sophistries that the "New Economic History" has foisted on the profession. Sadly, historians seem to be sinking into that enslavement to the deductive method and the static view of human affairs that long since brought economic theory to full impasse.

As to development of the idea of steam power itself, it was a long time (and many inventions later) before Watt's machine metamorphosed into the practical, efficient, and—very important—*small* machine of the second quarter of the nineteenth century. Metals, large casting techniques, and the art of machining them precisely, to mention a few factors, had to be innovated. A thousand little ropes pinned the Gulliver of steam to a few applications until the idea's time did come. Take, for example, the artist's conception of the Corliss vertical steam engine (the hit of the 1876 Philadelphia exposition) gracing the cover of the December 1982 *AHR*. A hundred and fifty years had passed since Newcomen's death, and still the state of the art was a reciprocating engine developing only 2500 horsepower, which nowadays some internal combustion locomotives develop while hurtling across the landscape at 70 miles an hour with 100 railroad cars in tow! Editors adore artists' drawings of the Corliss engine, probably because they exaggerated its dimensions, like a technological Petty girl. How many readers of the *AHR* ever saw a photograph of the Corliss engine? Quite a letdown. True, it was the marvel of its age and it took 150 years to get there; *but* barely twenty-five years later far more sophisticated reciprocal stationary engines were themselves being made obsolete by Parsons's turbine, which, in yet another twenty-five years, in greatly improved

form, was lighting the great cities of the world with electricity. To lose sight of all these contributory factors that brought a good idea to fulfillment is to forget what history is all about.

As to the infrastructure, it is, as any technological historian worth his salt knows, largely a story of the development of the steam railroad. Nowhere in Greenberg's article does she take up the obvious point that until we had cheap means of getting fuel to the places where it was most logical to erect factories, the steam engine remained tied to mine applications. Here we come to the most glaring flaw in Greenberg's article, which is her omission (inadvertent I trust) of any mention of Alfred D. Chandler, Jr.'s seminal article, "Anthracite Coal and the Beginnings of the Industrial Revolution in the United States" (*Business History Review*, 46 [1972], 141), in which it is demonstrated that not until the early anthracite district railroads brought the price of this fuel (by no means the best steam-making coal) down to a fraction of its canal-era price were manufacturing plants of the size and complexity that we associate with the industrial revolution possible. *Quod erat demonstrandum*, both for America and, I suspect, Great Britain as well.

Clearly, no mature scholar is going to waste his time arguing whether steam technology was an input to railroads or railroads were an input to steam technology. In history, "everything is happening at once," and the chicken-or-egg debate has no place. What historians *must* do, however, if they wish to understand the highly dynamic nature of economic development, is to begin to think a little more abstractly about what the "industrial" revolution was. As Chandler demonstrated better than anyone else up to this time (and with the help of dozens of earlier scholars) in his *Visible Hand: The Managerial Revolution in American Business* (1977), the first stage in the development of modern industry was transportation. Why historians insist on treating transportation (granted, an intangible service rather than a tangible good) as separate, conceptually, from manufacturing, can only be explained by their failure to think abstractly. (See, especially, Greenberg's article, pages 1257–58). Transportation is as much an input to manufacturing as the raw materials or semifinished goods that the output itself consists of. Even economists know that transportation contributes to "value added." Yet Greenberg blandly offered the fact that more than 50 percent of steam horsepower in England in 1850 was used to propel locomotives (page 1253), further belittling the industrial importance of steam.

Another nonfact in this article is Greenberg's comment that "animate sources continued to supply more than half of [United States] growing horsepower until 1880" (page 1241). But of course.

Nearly everybody lived on farms, while the steam tractor was far too large a "power package" for all but a few. All vehicular transportation in 1880 was still by animal power. Except for a small percentage accounted for by steam commuter railroads, *all* short-distance transportation, urban, town, or rural, was animal-hauled. (The steam rapid transit systems were just being built and the electric streetcar, when it came, used electricity generated by steam). As many Americans still living who remember the "white wings" on city streets can attest, animals still consisted of more than cats, dogs, and parakeets.

Despite the innumerable conferences that technological and energy historians are privileged to attend and participate in, which inevitably generate lengthy philosophical discussion, Greenberg showed a remarkable insensitivity to some of the most fundamental paradigms of economic development history. Her primary purpose—and this is perhaps where she used Clio neither wisely nor well—seems to be to prove that "soft" energy sources were once all any reasonable society needed, and that we can go back to them any time we get religion. Such objectives have no place in history, however trendy they may be.

ALBRO MARTIN
Bradley University

PROFESSOR GREENBERG REPLIES:

I am not at all certain what Albro Martin's invective is really about, unless it be his evident distress at my focus on the parallel and prolonged Anglo-American dependence on conventional power supplies. His claims as to my "primary purpose" can hardly be substantiated. I neither imply nor set out "to prove that 'soft energy' sources were once all any reasonable society needed" or, as he puts it, "that we can go back to them any time we get religion." These are Martin's polemical "straw men" and they are far removed from the premises of my essay. On the contrary, I was concerned with the relative contributions of all types of power to economic growth to qualify what have been unwarranted assumptions about the pace of steam adoption and the significance of this single source. As I indicated, only by "probing the relative impact of different types of power on economic development as societies emerge from preindustrial, handicraft origins" can we gain a better understanding of the connections between energy and the larger patterns of socioeconomic change.

Let us consider Martin's comments one by one. To begin with, this is not an essay in "technological history." My exploration of the gradual transition to steam belongs to the growing scholarship in energy history. Using concepts of energy analysis, it clarifies

the inadequacy of accepted propositions about the availability of power for economic expansion. In establishing a duplicate time-frame for the transatlantic transition from water to steam, moreover, I offered a new context for historical analysis. This Anglo-American comparison is clearly of no little interest since, as Martin must be aware, the periodization of the relationship between power and productivity during the Industrial Revolution has been a particular preoccupation of historians. And I hardly think it "unfair" to call attention to recent research on the increasing (and neglected) industrial reliance on animate and water power in England into the 1850s or to examine the reasons for the still-limited consumption of steam.

Martin's assault on the "New Economic History" for the "sophistries" it "has foisted on the profession" is as irrelevant as his criticism that "historians seem to be sinking into . . . enslavement to the deductive method." Surely we should be able to admit that too many generalizations have emerged from too little empirical information and that recent scholarly efforts have illuminated what is still an imperfect gauge of the divergence between what was hoped for and what happened in the century after the introduction of Watt's engine.

Martin chooses to bypass the disparity between the promise of steam and the century-long barriers to its diverse application. It is the nature of these constraints that are detailed as a central part of my argument to explain why steam did not "flourish" and why the "infrastructure," to use Martin's term, encouraged the growing reliance on traditional power sources. Boulton's expectations of the demand for steam and Jefferson's of its "extensive consequences" are evidence of an initial enthusiasm that contrasted, we find, with the protracted delay in English mechanization and with the relatively small demand for steam, particularly outside of transportation, in both England and the United States until the 1850s—that is, until after the era we commonly associate with the Industrial Revolution. Indeed, if the artist's rendering of the Corliss display appears somewhat larger than life, it was because, as I proposed, the idea of steam had been culturally blown out of proportion. But, in fact, since this Corliss Double Walking-Beam Steam Engine stood thirty feet high, the grandest structure in Machinery Hall, the more seriously distortive puffery I find is in Martin's rhetoric, with its analogy to Petty girls and its peculiar imputation that I ignored the contributions of technological development.

Martin presumably knows that I would be the first to concede the importance of railroads to U.S. institutional change after the 1840s (see my *Financiers and Railroads* [1980]); but unless one especially favors Rostow's "take-off" theory, it is difficult, in

assessing the Industrial Revolution, to give credence to Martin's statement that "the infrastructure . . . is . . . largely a story of the development of the steam railroad," once we acknowledge the strong continuous growth rates in manufacturing from as early as 1810. The eventual significance of steam for factory production should not be allowed to obscure the impressive ongoing contributions of water-powered operations, many of considerable complexity and increasing efficiency, to the economic development of the first half of the century.

Martin has misunderstood, or misrepresents, the issues. Clearly, the matter at hand is to establish the relatively limited applications of steam in English manufacturing as a corrective to presumptions of its widespread rapid adoption and significant institutional impact. No one doubts U.S. reliance on water or the linkages between delayed anthracite use and factory organization that Chandler so stunningly elucidated in his seminal study (cited as part of an extensive literature on fuels in my earlier examination of "Energy Flow in a Changing Economy, 1815–1880," *An Emerging Independent Economy* [1980]). What I addressed in the *Review* article are faulty suppositions about the marked differences in Anglo-American power choices before 1850, pointing instead to a notable congruence.

Investigating the relationship of power to the GNP in no way that I can see impairs our ability to think abstractly about "what the 'industrial' revolution was." A glance at my footnotes reveals that the concept of Energy-GNP ratios is an essential theoretical component of much of current economic and energy analysis. What we need, as I suggested, are various historical estimates. Yet we should note that, in calculating energy consumption, the value-added approach—so important to eliminating double counting of the GNP—is inapplicable. These energy data emerge from a wholly different conceptual apparatus, one that encompasses losses in physical energy conversion in each step from fuel extraction to ultimate end-use, and such figures are prepared by major sector, separating transportation from mining and manufacturing, to allow for a structural analysis of energy efficiencies.

Quite frankly, I do not know why Martin considers prolonged dependence on biological energy sources a "nonfact." I remain convinced that historians have inadequately accounted for the significance of the power produced by people and animals, not only in the late nineteenth century but for most of our history. But, perhaps, Martin and I do not share similar perceptions of what constitutes an interesting historical problem.

DOLORES GREENBERG
Hunter College
City University of New York

TO THE EDITOR:

Having read Hugh Cunningham's review of Samuel Pyeatt Menefee's *Wives for Sale: An Ethnographic Study of British Popular Divorce* (*AHR*, 88 [1983]:109–10), I can only wonder how the *American Historical Review* could allow a review with such antifemale bias to be published.

In Cunningham's last paragraph he states: "Wife selling was an 'informal institution'. . . . It was appropriate in a society in which one form of control had broken down and another—that of the bureaucratic state—had yet to arise." I find the use of the word "appropriate" rather insensitive. "Unavoidable" or "conceivable" might, under the circumstances, be more fitting ways to describe the evolution of this practice. But using the word "appropriate" to describe wife selling signifies a value judgment—that of tacit approval of this form of slavery—on the reviewer's part. That these wives, with few rights of their own, "frequently agreed to the sale" does not validate calling the practice "appropriate." Many of today's wives who turn up in battered women's shelters, for example, might gladly be sold to a more "benign" husband.

Readers of this review, and especially female readers, will undoubtedly find it offensive.

LESLEY A. RIMMEL
Political Science Quarterly

TO THE EDITOR:

The aim of Menefee's book is to account for the rise and fall of wife selling in Britain. He does so, convincingly, by linking the practice to the spread of a market economy. I described it as "appropriate" in that type of society in the sense of "belonging as an attribute" (*Oxford English Dictionary*). I certainly intended thereby no tacit approval, but I must also say that, on the evidence of Menefee's book, it does not help the understanding of wife selling to equate it with either slavery or wife battering. For the record I find all three abhorrent. As historians, however, we are called upon not so much to condemn, as to analyze the reasons both for the long-term subordination of women to men, and for the particular forms that it took in particular societies at particular times; to this latter issue Menefee's book makes a substantial contribution, and my review was intended to convey that fact.

HUGH CUNNINGHAM
University of Kent at Canterbury

TO THE EDITOR:

In his review of Jason Berger's *A New Deal for the World: Eleanor Roosevelt and American Foreign Policy*

(*AHR*, 87 [1982]: 1193), Thomas M. Campbell writes: "[Eleanor] Roosevelt's overly zealous backing of Israel was her chief shortcoming, as she generally spoke out in the Eisenhower years for broad global humanitarian understanding." Manifestly, this is Campbell's personal prejudice. His remark tells us nothing about Berger's scholarly book, and adds nothing to the historical record. The interests of professionalism would better be served if the book review editor exercised his red pencil somewhat more vigorously against those who seek to use an *AHR* book review for a soapbox.

JONATHAN D. SARNA
*Hebrew Union College—
Jewish Institute of Religion,
Cincinnati*

PROFESSOR CAMPBELL REPLIES:

Books hopefully add to the historical record Jonathan D. Sarna is so concerned about where reviews comment on the strengths and shortcomings of an author's contributions in understanding past events. Eleanor Roosevelt stands as perhaps the most influential American woman so far in this century, and anyone wanting to comprehend her abilities and many contributions would certainly want to read Berger's brief appraisal of her impact on foreign policy, along with other biographical studies. One of the chief threads in Berger's account is to show Eleanor Roosevelt's long interest in and efforts to aid the Jewish people so horribly engulfed in the Nazi extermination mania. As the president's wife and as an outspoken leader in her own right, Roosevelt fought to overcome the callousness of existing American immigration restrictions that the State Department seemed all too bent on upholding. It was a battle with few successes, and when Rabbi Stephen Wise appealed to her in late 1942 to help publicize the role of German concentration camps, she replied that "I have written and spoken on the subject and do not know what more I can say" (p. 31).

By that time Roosevelt was occupied with many more issues brought on by America's involvement in the war. But behind her sense of helplessness conveyed to Rabbi Wise was probably a significant feeling of guilt that was shared by large numbers of non-Jewish citizens in the Allied nations, that so little could be done to halt the disaster befalling European Jews. After her husband's death, circumstances developed that enhanced Roosevelt's influence on American foreign policy during the Truman years. The new president looked to Eleanor Roosevelt to take a leading role in securing the international community's adherence to the Universal Declara-

tion on Human Rights. Coverage of her leadership in winning approval of the Declaration is one of the strongest parts of Berger's work. At the same time Roosevelt emerged as a major liberal spokesperson against the threats to humanitarian values posed by the Soviet Union.

This was the setting in which she exerted influence on Truman in the tangled circumstances culminating in the United States' quick recognition of the state of Israel. Roosevelt, as Berger points out, feared that failure to act quickly on recognition might lead the new nation to turn for succor to the Soviets. One of the chief elements in her support for American aid to Israel and her own defense of Israeli policies and actions, as clearly delineated in Berger's book, was her persistent conviction that Israel offered one of the few bulwarks in the Middle East against Russia's drive to penetrate the region as part of the Kremlin strategy of winning influence in Africa and India.

Perhaps it was her loathing of McCarthyism that made her highly critical of the foreign policy initiatives of Eisenhower and Dulles. Whatever the source, Eleanor Roosevelt stands in Berger's study as an unhesitant apologist of Israel in the 1950s. Often comparing Israel to frontier America, she explained Israeli actions as defensive amid hostile neighboring Arab threats. Thus, she saw Egypt's Nasser as simply a Soviet pawn. When Israel attacked Egypt in October 1956, and Britain and France joined within a few days, Roosevelt excused Israeli aggression as necessary because of the Arab determination to destroy Israel's democratic nation-building efforts. When the Eisenhower administration joined Russia in the United Nations in demanding a halt to the attack against Egypt, she castigated the government for abandoning its Western allies and furthering the sinister aims of Moscow.

Berger shows Roosevelt as a dedicated leader in a global campaign to win international support for human rights, but his study makes quite apparent that, at the same time, she had little sympathy for the rights and claims of Palestinians if they conflicted with interests of Israel. That she should take a partisan stand regarding Israel is entirely understandable in light of her warm friendships among the American Jewish community and her own burning conviction of the need for a Jewish homeland after seeing such staggering suffering of Jews during World War II.

In closing, I will admit my view is that the United States has pursued an uneven policy regarding Israel and the Palestinians where our own national interests would be better served by a balanced approach to Arab nations and Israel. At the same time, my professional standards are such that I have never regarded reviewing books as a proper place

for a soapbox; and I think Sarna is the person exerting personal passion where it has no place.

THOMAS M. CAMPBELL
Florida State University

TO THE EDITOR:

John Higham's puzzling review of *Ethnic America* was the kind that makes you wonder what book he was reading—and what axe he was grinding.

Despite an abundance of generalities about my "errors," "groundless statements," "reckless assurance," and lack of "rigor," Higham comes up with a grand total of *one* fact that he challenges—the occupations of the Irish in the 1890s—and even here he gives no reason whatsoever for his challenge.

He accuses me of "making light of racial discrimination in the North before the great black migrations from the South." Yet anyone who turns to page 210 of *Ethnic America* will find me referring to the "hostility" faced by Northern blacks in that era, and to discriminatory laws and practices in the North, including "barring black children from schools and black adults from equal access to public accommodations." Is that "making light"? *Ethnic America* then lists documented, empirical evidence from a variety of sources that some progress in such attitudes and practices had begun—only to end abruptly and revert to rigid racial barriers with the great migrations. Again, Higham offers no facts to the contrary.

One of the points emphasized in *Ethnic America* is that acculturation has been a two-way street, that the various groups changed America as well as being changed by it. This point is made again and again in discussions of particular groups and is repeated on page 286 of the summary chapter. Yet Higham accuses me of "inattention" to the fact that ethnic groups "have not merely responded to America, they have also changed it."

Something much worse than "inattention" seems to be involved in Higham's repeated distortions. One clue may be his warning at the outset of his review that I have "ideological predilections"—as those who think as he does presumably do not. But does that square with a review whose virulence is exceeded only by its vagueness?

THOMAS SOWELL
Hoover Institution

PROFESSOR HIGHAM REPLIES:

I regret that Thomas Sowell found my review virulent and hope that others did not. I am happy to have the occasion to say again that *Ethnic America* is

an instructive, densely informed book, which moves toward—though it does not in my opinion reach—a much-needed reformulation of the subject.

Here are some factual slips I noticed:

The statement that 42 percent of “the Irish” were still servants in 1890 (p. 35) rests on a misunderstanding of a misleading secondary source. The census reports that 16 percent of the gainfully employed Irish of foreign birth were servants.

American descendants “of people from the British Isles” now number far more than 29 million (p. 43). Already in the 1920s, when the only serious study of descent groups was made, 42 million Americans supposedly derived from England, Scotland, and Wales.

The Jews of eastern Europe were not demonstrably poorer than “any other immigrant group” at the time of their arrival in the U.S. (p. 80). Sowell’s source says that they had less cash per person than most other immigrants, but even that figure may be misleading in comparing a family migration with a migration of single workers. It also leaves out of account the well-to-do Jews who traveled cabin class and avoided altogether the inquisitive inspectors at Ellis Island.

The twice-repeated statement that the U.S. government during World War I prosecuted American-Jewish newspapers for pro-German statements (pp. 63, 97) is a distortion of a single unsuccessful effort to suspend the mailing privileges of a pacifist, socialist newspaper, the *Jewish Daily Forward*.

Sowell claims too much in making the reduction and partial elimination of intergroup animosities a “peculiar” American achievement (p. 9). Many other societies have enjoyed long periods of accommodation or assimilation between ethnic groups.

Any well-informed reader of *Ethnic America* can make his own list of such items. They are mostly of little consequence and not worth reviewing here. For the most part Sowell’s facts, taken one by one, are correct. A more serious question is whether a satisfactory general history of American ethnic groups can rest essentially on an assessment of their relative “success” in rising from rags to riches. That is what Thomas Sowell and I disagree mainly about.

JOHN HIGHAM

Johns Hopkins University

TO THE EDITOR:

In his review of *The Urban Establishment* by Frederic Cople Jaher (*AHR*, 88 [1983]: 167), Thomas Bender writes that what he calls this “rather insignificant” and unsuccessful book “raises some hard questions about Edward Pessen’s statistical findings on New York and Boston.” In defense of my “findings” and

the work on which they are based, may I call attention to my review in *New York History* (63 [1982]: 482–85), which deals with and answers these questions?

EDWARD PESSEN

*Baruch College and
Graduate School and University Center
City University of New York*

TO THE EDITOR:

The review of Alfred E. Senn’s recent book, “Assassination in Switzerland: The Murder of Vatslav Vorovsky,” published in the October issue (*AHR*, 87 [1982]: 1126), contains an inaccuracy that could mislead historians interested in the subject. I was astonished to read Teddy J. Uldrick’s statement according to which Senn was the one who “has painstakingly reconstructed (the) whole episode”. It is not without reluctance that I draw your attention to the fact that the reconstruction of the episode was done in a scientific manner (the 1,155 annotations indicate each source: books, articles, private and public archives, interviews and correspondence with persons involved in the affair and living in several countries) in 1975 in my doctoral thesis “L’affaire Conradi” accepted by the University of Zurich and published by Herbert Lang.

ANNETTA GATTIKER
Berne, Switzerland

PROFESSOR SENN REPLIES:

Dr. Gattiker’s statement does not of itself require an author’s “reply,” but lest silence be misinterpreted, I wish again, as I did in my book, to express my indebtedness both to her book and also for her help while I was working on my manuscript.

ALFRED E. SENN
*University of Wisconsin,
Madison*

TO THE EDITOR:

Book reviews are critical events not only for the ego of an author but also for the future of a discipline or subdiscipline, for they are discursive trials that help to shape the structure of research by meting out penalties or rewards. In the latter respect, James Smith Allen’s recent review of my book, “*Madame Bovary on Trial*,” raises a number of issues that I would like to address briefly. I think Allen misreads or even tends to be dismissive toward my book because he applies the “paradigm” for a variant of social history to what is openly and avowedly an

attempt to reformulate our conception of intellectual history. There is much value in the social history of ideas. But it is not the approach I take in my book. Nor does it define the only valid approach to intellectual history or the norm according to which all other approaches should be judged. What I argue is that the intellectual historian must supplement the documentation of empirical processes of reception with a critical reading or interpretation of texts in order to acquire some understanding of what occurs in those empirical processes of reception. Thus the full-scale documentation of the empirical reception of the novel by various social groups or categories is not the objective of my book. Its stated objective is rather to explore what I see as the significant tension between critical and even uncanny ways the novel treats crucial social norms and the manner in which the prosecution and the defense at the trial perhaps necessarily misread the novel so that they can employ the very norms it calls into question in order to judge it. I do not claim that the reading at the trial is "typical" of the nineteenth century. On the one hand, I think that it is in certain ways still prevalent today, even at times among historians. On the other hand, I realize full well that, if an assertion of this sort is to be more than impressionistic for any time or place, it requires the kind of research I did not undertake. The most I say with reference to the typical nature of the reading at the trial is the following: "To the extent that forensic rhetoric is based upon an accurate appreciation of the expectations of an audience that lawyers attempt to convince or to persuade, a trial may also be an index of conventions or norms of reading in the larger public, at least on an 'official' level of consciousness" (pp. 15–16).

To defend my argument that *Madame Bovary*, largely in view of its "style" (notably the use of the so-called *style indirect libre*), may be interpreted as a special form of "ideological crime," I undertake a close reading of the novel and an investigation of how my interpretation relates to those of notable critics and commentators, especially Jean-Paul Sartre. Allen finds all this irrelevant. Perhaps it may be for his purposes (although one may argue this point), but it certainly is not for mine. Indeed my concern is that historians, including intellectual historians, are not trained to read. They may even be professionally trained not to read by being induced to confide in a narrowly documentary model of knowledge and to believe that an appeal to "the context" solves all problems in historical interpretation. An ill-defined notion of "the context" may well serve as a pretext to avoid or to get around the problem of reading and interpreting complex texts. I feel that intellectual historians must undertake more careful and limited inquiries into precisely how texts internalize or repel various contexts and,

conversely, how various contexts respond to texts. My book was intended as such an effort.

It is altogether implausible to believe that archival research could possibly answer the questions I raised about the reading of *Madame Bovary* at Flaubert's trial. Of course, this is not to say that it would not be useful to have lawyers' briefs or memoranda from the Ministry of the Interior to broaden the spectrum of instances of empirical reception. But it is radically to contest the idea that the only questions worth asking in historiography are ones that can indeed be answered by archival research. As H. Stuart Hughes recently put the point: "Historians in this country seem to have forgotten—if they ever learned properly—the simple truth that what one may call progress in their endeavors comes not merely through the discovery of new materials but at least as much through a *new reading* of materials already available" ("Contemporary Historiography: Progress, Paradigms, and the Regression Toward Positivism" in *Progress and Its Discontents*, ed. Gabriel A. Almond, Marvin Chodorow, and Roy Harvey Harris [1982], p. 245). Here I would simply note that, to the best of my knowledge, the kind of reading of Flaubert's trial that I attempted has never been undertaken before, and the approach I take can certainly be applied to other instances of reception. I should nonetheless have indicated that, according to inquiries I did make, such archival material as referred to by Allen either does not exist or was destroyed during the Paris Commune. I should also have explicitly stated that I did not compare the trials of Flaubert and Baudelaire because it would have been superfluous to an argument I tried to make as economically as possible. The trial of Baudelaire was a reduced replica of the trial of Flaubert with the same prosecuting attorney (Pinard) making abbreviated versions of the same points. *Ça se répète*, as the French might say. Finally, it could hardly be said that I "slight" the journal of Ernest Pinard. Not only do I refer to its relevant discussion of Flaubert's trial, but the fact that its retrospect involves distortions egregious enough to make one suspect the role of defense mechanisms is itself crucial in lending plausibility to the interpretation I offer.

It often seems to me that the saying that history has many mansions is little more than a monumental bromide. History ranges from mansions to shacks, thereby paralleling the society in which it exists. Today social history tends to occupy many of the mansions and intellectual history a number of the shacks. My own approach to intellectual history is an attempt to demonstrate that ruling ideas need not rule with uncontested hegemony. One may adduce any number of arguments against this approach, and in the unlikely event that it ever threatens to become dominant in the subdiscipline (much less in the discipline as a whole), I hope to be among

the first to employ some of them. But one should confront this approach head on rather than through the misleading device of dismissing it by invoking an inappropriate protocol of method in social history.

DOMINICK LACAPRA
Cornell University

PROFESSOR ALLEN REPLIES:

Twelve years ago Felix Gilbert wrote, "[Intellectual history] exists only in connection with, and in relation to, the surrounding political, economic, and social forces. The investigation of subjects of intellectual history leads beyond the purely intellectual world, and intellectual history per se does not exist. But if this is so, has the intellectual historian a special task, and what is it?" ("Intellectual History: Its Aims and Methods" in *Historical Studies Today*, ed. Gilbert and Stephen Graubard [1972], p. 155). Gilbert's observations and questions about this beleaguered field are still pertinent. I believe they highlight the issue I have with Dominick LaCapra's approach to history suggested in his letter; one he has sketched more fully in a recent essay ("Rethinking Intellectual History and Reading Texts" in *Modern European Intellectual History*, ed. Dominick LaCapra and Steven Kaplan [1982], pp. 47–85) and illustrated by his "*Madame Bovary*" on Trial. By focusing on a laudably self-conscious but regrettably selective treatment of texts, LaCapra has marked out an intellectual history that is more intellectual than history. He is clearly more concerned about the relationship between the reader and the text than he is about the relationship between the historian and the past. While LaCapra's concerns with a process of inquiry may be interesting theoretically, they do not shed much light in practice; such critical reading is neither a historical methodology nor any historical knowledge.

For a reviewer to state this is not to stake out a new and expanded claim for social history. Far from it. Rather, it is to call attention to the wider range of historical activity involved in reading both works and documents. The social world of author and audience is but one focus of the historian's attention; besides texts and discourses, the economic, the political, the religious, and the literary, in the past and over time, are others well worth investigating in intellectual history. LaCapra, however, has urged historians to examine how texts respond to specific contexts that are in essence, I believe, too constricted and ahistorical. With its emphasis on recent readings of Flaubert's *Madame Bovary*, LaCapra's book deals all too little with strictly historical concerns, including those that LaCapra himself admitted in his article are important, namely, the intentions, motivations, society, culture, corpus, and structure of the text ("Rethinking," pp. 57–78). Instead, his

book considers more Flaubert's novel as a critical literary event experienced by a very select list of readers, many of them quite recent. As a historian interested in readings as much as readers, past and present, I found this particular focus unrevealing both methodologically and historically, however insightful critically.

Nor am I entirely convinced by LaCapra's assurances about the claims he makes (or does not make) in his book. He says more about the "typical" nature of the reading at the trial than he actually quotes in his letter: "In any event, a trial enables one to be somewhat more precise in investigating a 'mentalité' or 'climate of opinion'" (p. 16). That LaCapra argues an ideological crime implicit in the novel's style suggests that the trial (mis)read the text in the same way middle-class readers did during the Second Empire, whose bourgeois norms of family and religion were violated. I am not convinced either that archival research would not help answer the questions LaCapra raised about the reading of *Madame Bovary* at Flaubert's trial. While the Interior Ministry's actual dossier may indeed have been destroyed during the last week of the Paris Commune, relevant Ministry records worth exploring still exist in the Archives Nationales (F¹⁸ 312–426). Portions of Baudelaire's dossier are also extant (BB²¹ 631–34) and would illuminate the manner in which other officials besides Pinard read texts. Certainly Pinard's journal deserves more attention than LaCapra's use of André Pasquet's *Ernest Pinard et le procès de Madame Bovary* (1949).

But these questions involving the historian's craft are less important than the more substantive issue of the historian's domain. This difference between LaCapra and myself reminds me of Oscar Handlin's metaphorical account of the profession, "Living in the Valley" (in *Truth in History* [1979], pp. 25–39). There Handlin describes, somewhat wistfully, the departure of many talented historians from the plains of history for the new disciplinary perspectives offered from the surrounding mountain tops. LaCapra apparently has left history's many mansions, hovels and châteaux alike, in the valley for new climes in literary theory. And perhaps like many others before him venturing into other disciplines, he will return with new treasures to reconstruct his old home. One cannot but be impressed by the results of other intellectual historians, discussed in Robert Darnton's contribution to *The Past Before Us* [1980], pp. 327–48), who have maintained the relationship between the "critical" intellectual and the "scholarly" historian. Unfortunately, for reasons I have stated here and in my review, I believe LaCapra's "*Madame Bovary*" on Trial is not one of them.

JAMES SMITH ALLEN
Phillips University

American Historical Association

Founded in 1884. Chartered by Congress in 1889
Office: 400 A Street, S.E., Washington, D.C. 20003

President: Philip D. Curtin, *Johns Hopkins University*
President-elect: Arthur S. Link, *Princeton University*
Executive Director: Samuel R. Gammon
Controller: James H. Leatherwood

MEMBERSHIP: Persons interested in historical studies, whether professionally or otherwise, are invited to membership. The present membership is about 15,000. Members elect the officers by ballot.

MEETINGS: The Association's annual meeting takes place December 28–30. The meeting in 1983 will be held in San Francisco. Many professional historical groups meet within or jointly with the Association at this time. The Pacific Coast Branch holds separate meetings on the Pacific Coast and publishes the *Pacific Historical Review*.

PUBLICATIONS AND SERVICES: The *American Historical Review* is published five times a year and sent to all members. It is available by subscription to institutions. The Association also publishes its *Annual Report*, *AHA Perspectives* (newsletter with classified listings), a variety of pamphlets on historical subjects, the bibliographic series *Writings on American History*, and *Recently Published Articles*. To promote history and assist historians, the Association offers other services, including an Institutional Services Program. It also maintains close relations with international, specialized, state, and local historical societies through conferences and correspondence.

PRIZES: The *Herbert B. Adams Prize* of \$300 awarded annually for a first book in the field of European history. The *George Louis Beer Prize* of \$300 awarded annually for a book on any phase of European international history since 1895. The *Albert J. Beveridge Award* of \$1,000 given annually for the best book on the history of the United States, Canada, or Latin America. The *Albert B. Corey Prize*, sponsored jointly by the AHA and the Canadian Historical Association, of \$2,000 awarded biennially for the best book on the history of Canadian-American relations or the history of both countries (next award, 1984). The *John H. Dunning Prize* of \$300 awarded in the even-numbered years for a book on any subject relating to American history. The *John K. Fairbank Prize in East Asian History* of \$500 awarded in the odd-numbered years. The *Leo Gershey*

Award of \$1,000 awarded in the odd-numbered years for the most outstanding work in seventeenth- or eighteenth-century European history. The *Clarence H. Haring Prize* of \$500 awarded every five years to that Latin American who has published the most outstanding book in Latin American history during the preceding five years (next award, 1986). The *Howard R. Marraro Prize* in Italian history awarded annually and carrying a cash award of \$500. The *James Harvey Robinson Prize* for the teaching aid that has made the most outstanding contribution to the teaching of history (next triennial award, 1984). The *Robert Livingston Schuyler Prize* of \$500 awarded every five years for the best work in modern British and Commonwealth history (next award, 1986). The *Watumull Prize* of \$1,000 awarded in the even-numbered years for a work on the history of India originally published in the United States. The *J. Franklin Jameson Prize* awarded every five years, for outstanding editorial achievement in the editing of historical sources (next award, 1985). The *Waldo G. Leland Prize* awarded every five years offered for the most outstanding reference tool in the field of history (next award, 1986). The *Alexis de Tocqueville Prize* offered every five years for the best work in U.S. history published outside the United States by a foreign scholar in any language (next award, 1984).

DUES: For incomes over \$30,000, \$50.00 annually; \$20,000–29,999, \$42.00; \$15,000–19,999, \$35.00; \$10,000–14,999, \$25.00; below \$10,000 and joint memberships, \$15.00; associate (nonhistorian) \$25.00; life \$1,000. For all annual memberships, add a postage surcharge of \$2.50 (domestic) or \$5.00 (foreign). Members receive the *American Historical Review*, *AHA Perspectives*, the program of the annual meeting, and the *Annual Report* on request and may subscribe to the *RPA* for \$12.00 (plus a \$1.00 postage surcharge for foreign).

CORRESPONDENCE: Inquiries should be addressed to the Executive Director at 400 A Street, S.E., Washington, D.C. 20003.

1(a)

American Historical Review

Founded in 1895

The *AHR* is sent to all members of the American Historical Association; information concerning membership will be found on the preceding page. The *AHR* is also available to institutions by subscription. There are five categories of subscription:

CLASS I: *American Historical Review* only, United States, Canada, and Mexico \$43.00, foreign \$47.00.

CLASS II: *American Historical Review*, *AHA Perspectives*, the program of the annual meeting of the Association, and the *Annual Report*, United States, Canada, and Mexico \$54.00, foreign \$60.00.

CLASS III: Subscription to *Recently Published Articles* only, \$22.00, overseas postage add \$2.00.

CLASS IV: *American Historical Review* with *Recently Published Articles*, United States, Canada, and Mexico \$61.00, foreign \$66.00.

CLASS V: *American Historical Review*, *AHA Perspectives*, the program of the annual meeting of the Association, and the *Annual Report*, with *Recently Published Articles*, United States, Canada, and Mexico \$72.00, foreign \$79.00.

Single copies of the current issue and back issues in and subsequent to volume 82 (1977) can be ordered from the Membership Secretary of the Association at \$10.00 per copy. Issues prior to volume 82 should be ordered from Kraus Reprint Corporation, Route 100, Millwood, N.Y., 10546.

Correspondence regarding contributions and books for review should be sent to the Editor, *American Historical Review*, 914 Atwater, Indiana University, Bloomington, Indiana 47405. Unsolicited book reviews are not accepted; a statement concerning the kinds of articles the *AHR* ordinarily will and will not publish appears in the issue for October 1970 (75:1577–80). No manuscript will be considered for publication if it is concurrently under consideration by another journal or press or if it has been published or is soon to be published elsewhere. Both restrictions apply to the substance as well as to the exact wording and the language of the manuscript. If the manuscript is accepted, at least one year must elapse between publication in the *Review* and republication of the essay, or any significant part thereof, in another work. The entire text, including quotations and footnotes, of article manuscripts must be submitted in double-spaced typescript, with generous margins to allow for copyediting, and submitted in duplicate. Footnotes should be numbered consecutively throughout and should appear in a separate section at the end of the text. Other guidelines for the preparation of manuscripts for submission to and publication in the *AHR* will be sent upon request. Articles will be edited to conform to *AHR* style in matters of punctuation, capitalization, and the like; and the editors may suggest other changes in the interest of clarity and economy of expression. But such changes are not made without consultation with authors. The editors are the final arbiters of length, grammar, usage, and the laws of libel.

2(a)

Books that matter are Basic

The Shadow Warriors

O.S.S. and the Origins of the C.I.A.

Bradley F. Smith

Here is the definitive account of the nation's first intelligence agency—how it operated, what it really accomplished, and how it laid the basis for the present Central Intelligence Agency. Only now, with the release of thousands of previously classified documents, can the complex and fascinating story of the O.S.S. be told in full. "A monumental, authoritative history . . . head and shoulders above all other accounts. An important, cautionary book that reads like a high-grade spy thriller. It will keep you turning pages far into the night."

—*John Toland*

\$20.75

Spheres of Justice

A Defense of Pluralism and Equality

Michael Walzer

One of the nation's leading philosophers looks at how societies distribute the full range of goods—not only wealth and power but honor, work, education, free time, and even love. "This is a fascinating and important book: Walzer presents a sophisticated new theory of equality and supports it by surveying an extraordinarily rich array of social and political ideologies and arrangements, past and present." —*Judith Jarvis Thomson, M.I.T.*

\$19.95

Tolstoy and Gandhi, Men of Peace

A Biography

Martin Green

In the great tradition of exemplary "lives," Martin Green recreates two heroic—even saintly—lives: Tolstoy's and Gandhi's. Green vividly relates how two dissimilar figures, from "distant" traditions—the Russian aristocrat and novelist, the Indian lawyer and political protester—gradually reached the same imperative: utter renunciation of the imperialistic, warlike modern Western world system. This beautifully written work is a biographical tour de force, shifting skillfully from Tolstoy to Gandhi at various phases of their lives and encompassing all aspects of their respective worlds.

\$23.50

Basic Books, Inc.

10 East 53rd Street, New York, N.Y. 10022



ANNOUNCING . . . a publishing
“landmark” in American history
Volume 1 of
The Journals of the Lewis and Clark Expedition



Atlas of the Lewis and Clark Expedition

Edited by Gary E. Moulton

- The first definitive collection of the maps produced on the Lewis and Clark expedition
- 134 maps, 118 produced at full original size
- 42 maps never before published
- Introductory essay on the cartographic history of the expedition
- 196 pages, 13 x 19½ inches, \$100.00

“The *Atlas* will be a **landmark in Lewis and Clark scholarship** for years to come.”

—John Logan Allen, *University of Connecticut*

“Anyone interested in Lewis and Clark will find the new *Atlas* **invaluable**.”—*Alvin M. Josephy, Jr.*

“For most western scholars, the *Atlas* will be **the most important book published for several decades**.”

—*W. Raymond Wood, University of Missouri*

The Journals of the Lewis and Clark Expedition

Edited by Gary E. Moulton

- Sponsored by the American Philological Society and the Center for Great Plains Studies
- The first edition since 1904-05
- 11 volumes projected, ca. 500 pages each
- Definitive text with clarifying notes
- Included in this edition and not available in the first are: Lewis and Clark's 1803 journal from Pittsburgh to St. Louis; Clark's field notes of 1803-1805, discovered in the 1950s; Sergeant John Ordway's journal; Private Joseph Whitehouse's paraphrased journal, unknown until the

1960s; Sergeant Patrick Gass's journal; many additional manuscripts either unknown or not printed in the first edition

“For nearly 180 years the world has needed a **complete and modern edition** of the journals of Lewis and Clark. This one promises to answer that need.”—*Donald Jackson, Editor, Letters of the Lewis and Clark Expedition*

“This project is **one of the monumentally important undertakings**, not only in Western history, but in American cultural history in general.”—*William Goetzmann, University of Texas*

**University of
Nebraska Press**

901 N. 17th Lincoln 68588



LSU PRESS

Building an Antislavery Wall

*Black Americans in the
Atlantic Abolitionist
Movement, 1830–1860*

R. J. M. Blackett

In *Building an Antislavery Wall*, R. J. M. Blackett examines the efforts of black Americans in England to advance the cause of their own freedom by creating a "moral cordon" around the United States.

\$25.00

Parties and Politics in North Carolina, 1836–1865

Marc W. Kruman

Kruman traces the survival of North Carolina's competitive two-party system throughout the era of secession and the Civil War, revealing the relationship of prewar and postwar state politics and isolating the nature of political change during the war.

\$37.50, cloth; \$14.95, paper

Renato Beluche

*Smuggler, Privateer, and
Patriot, 1780–1860*

Jane Lucas De Grummond

In tracing the course of Renato Beluche's chameleonlike existence, this biography by Jane Lucas De Grummond gives us a panoramic view of the complex affairs of the Caribbean during the most volatile period of its history.

\$27.50

Neither Peace nor War

*Franco-American Relations,
1803–1812*

Clifford L. Egan

Egan follows the uneasy course of diplomatic relations between France and the United States in the ten years following the Louisiana Purchase, showing how Napoleon attempted to force America into declaring war on Britain.

\$30.00

Napoleon III and the Concert of Europe

William E. Echard

Detailing the principles and workings of the Second Empire's foreign policy, Echard follows Napoleon III's diplomatic efforts from the early successes of the Congress of Paris and the Crimean War to the cataclysm of the Franco-Prussian War.

\$32.50

The People of England

*A Short Social
and Economic History*

Maurice Ashley

A vivid guide, *The People of England* elegantly and clearly explores England's social and economic history through twenty-five centuries of constant change and abrupt fluctuation.

\$20.00

Louisiana State University Press

Baton Rouge

70803

World History



From Hitler to Ulbricht

The Communist Reconstruction of East Germany, 1945-6

GREGORY W. SANDFORD

"This carefully researched and well-written book casts new light on the indigenous social and economic origins of the postwar order in East Germany..."—*Henry Krisch, The University of Connecticut*
\$25.00

Cloth Reissue

A History of Modern Germany

HAJO HOLBORN

[a] "masterly account of the dramatic, tragic and often shameful history of Germany... which will probably become the most widely read of Holborn's works."—*New York Times Book Review*

Volume 1: The Reformation
Cloth, \$37.50. Paper, \$7.95

Volume 2: 1648-1840
Cloth, \$50.00. Paper, \$8.95

Volume 3: 1840-1945
Cloth, \$65.00. Paper, \$9.95

Three-volume set: Cloth, \$135.00.
Paper, \$24.50

Peasant Classes

The Bureaucratization of Property and Family Relations under the Hapsburg Absolutism, 1511-1636

HERMANN REBEL

"An original contribution that will add much to the knowledge of early rural Europe and can also serve as a methodological model."

—*Jerome Blum, Princeton University*
\$32.50

Poland's Place in Europe

General Sikorski and the Origin of the Oder-Neisse Line, 1939-1942

SARAH MEIKLEJOHN TERRY

"The scholarship and research are impeccable and most impressive."

—*Andrzej Korbonski, University of California at Los Angeles*
Cloth, \$40.00. Limited Paperback Edition, \$15.00

Agrarian Elites and Italian Fascism

The Province of Bologna, 1901-1926

ANTHONY L. CARDOZA

"A very useful and suggestive study. The latent civil war that led to fascism is traced back in important respects to before World War I..."

—*Charles S. Maier, Harvard University*
\$42.50

Electoral Behavior in Unreformed England

Plumpers, Splitters, and Straights

JOHN A. PHILLIPS

"This is the best and most convincing account of late eighteenth-century political reality yet written."

—*Robert A. Smith, Emory University*
\$35.00

From Princeton

The Great Fear of 1789

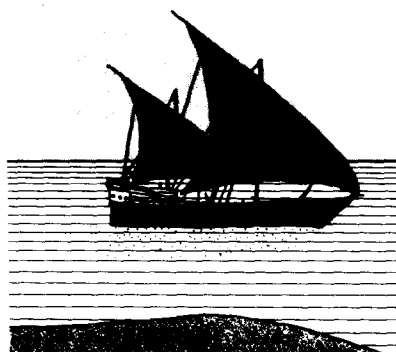
Rural Panic in Revolutionary France

GEORGES LEFEBVRE

Translated by JOAN WHITE

Introduction by GEORGE RUDÉ

"A masterful exploration of the social logic of the hysteria of 1789, *The Great Fear* remains, above all, a profound study in revolutionary myth."—*R.M. Andrews, New York Times Book Review*
Cloth, \$27.50. Paper, \$6.95



The Ottoman Slave Trade and Its Suppression

EHUD R. TOLEDANO

"Toledano's work opens a new area of study and will be of great value to those interested in nineteenth-century Ottoman history..."

—*Jere Bacharach, University of Washington*
\$28.50

The Old Social Classes and the Revolutionary Movements of Iraq

A Study of Iraq's Old Landed and Commercial Classes and of its Communists, Ba'thists and Free Officers

HANNA BATATU

"This is surely one of the most remarkable pieces of modern historical research I have read."

—*Robert Fernea, University of Texas, Austin*

Princeton Studies in the Near East

Cloth, \$100.00. Paper, \$27.50

Challenging Colonialism

Bank Mīsr and Egyptian

Industrialization, 1920-1941

ERIC DAVIS

"No student of the Near East, certainly of Egypt, can ignore this book..."—*Richard P. Mitchell, University of Michigan*

\$23.50

Conflict in Modern Japanese History

The Neglected Tradition

Edited by TETSUO NAJITA and

J. VICTOR KOSCHMANN

Sponsored by the Social Research Council and the Joint Commission on Japanese Studies of the American Council of Learned Societies

Cloth, \$42.50. Limited Paperback Edition, \$14.95

Princeton University Press

41 William Street Princeton, NJ 08540

FRENCH AND GERMANS, GERMANS AND FRENCH

A PERSONAL INTERPRETATION OF FRANCE UNDER
TWO OCCUPATIONS 1914-1918 / 1940-1944

Richard Cobb

An engaging "I was there" synthesis of a preeminent historian's recent research and original observations on wartime occupation. Cobb describes it as "an evocation drawing heavily on literary, or semi-literary, sources and even on autobiography, rather than a straight piece of history." *Tauber Institute Series*, 2. \$15.95

THE CHANGING OF THE GODS

Frank E. Manuel

Illuminates the author's latest thinking on religious belief and disbelief in the Enlightenment. *A Brown book*. \$18.00

GREAT WATERS

A HISTORY OF BOSTON'S WATER SUPPLY

Fern L. Nesson

"An interesting story convincingly told. Nonspecialists as well as historians of technology and politics will enjoy it"—Mark Rose, Michigan Technological University. *A Brandeis book*. \$15.00

Now in paper

THE EASTERN FRONTIER

THE SETTLEMENT OF NORTHERN NEW ENGLAND, 1610-1763

Charles E. Clark

"Exceedingly readable... a masterly description and analysis of the settlement and growth of a wilderness into what Hakluyt would have called 'civillitie'"—*American Historical Review*. *A UNH book*. \$15.00

THE QUEST FOR ETERNITY

MANNERS AND MORALS IN THE AGE OF CHIVALRY

Charles T. Wood

"The best image of the Middle Ages that is available"—*Speculum*. First published as *The Age of Chivalry*. *A Dartmouth book*. \$7.95

RACIAL MYTH IN ENGLISH HISTORY

TROJANS, TEUTONS, AND ANGLO-SAXONS

Hugh A. MacDougall

"Adds an important historical perspective to our knowledge of the origins and staying power of two pervasive myths... a fascinating account"—Gordon Fairweather, *Ottawa Citizen*. \$6.95



UNIVERSITY PRESS OF
NEW ENGLAND

HANOVER AND LONDON

The Papers of George Washington

Colonial Series

Volume I: 1748-August 1755

Volume II: August 1755-April 1756



W. W. Abbot, *Editor*

Dorothy Twohig, *Associate Editor*

Philander D. Chase, Beverly H. Runge, and Frederick Hall Schmidt,
Assistant Editors

The publication of the first two volumes of the *Papers of George Washington* marks the beginning of the first complete edition of Washington's correspondence. The ten-year search for Washington material has unearthed Washington documents in nearly three hundred American repositories and more than seventy foreign archives, and in addition several hundred private owners have contributed copies of Washington letters. The volumes which will contain virtually all extant Washington material will include letters written to Washington as well as letters and documents written by him. The correspondence volumes of the *Papers* will be divided into colonial, revolutionary and presidential series, with volumes in each series appearing concurrently. Standing orders for the *Papers* are welcomed.

Vol. I, \$25.00

Vol. II, \$25.00

THE UNIVERSITY PRESS OF VIRGINIA

Box 3608, University Station • Charlottesville, VA 22903

Delaware

General John Burgoyne by Richard J. Hargrove, Jr.

Winner of the 1980 University of Delaware Press competition for best manuscript on military, naval, and diplomatic history, *General John Burgoyne* offers a new interpretation of a favorite story. It is the first study that is both a scholarly biography of Burgoyne and an accurate, well-documented account of the 1777 expedition.

ISBN 200

294 pages

\$28.50

The Surpriser: The Life of Rowland, Lord Hill

by Gordon L. Teffeteller

Corrects and expands the dimensions of Rowland Hill's contribution to the Allied victory over Napoleon. This book also analyzes Hill's accomplishments as commander in chief of the army, a position he held from 1828 to shortly before his death in 1842.

ISBN 212

266 pages

\$32.50

Typhus and Doughboys: The American Polish Typhus Relief Expedition 1919-21 by Alfred E. Cornebise.

Describes the U.S. Army's relief expedition of 500 officers and enlisted men that helped combat the virulent epidemic plaguing Eastern and Central Europe. The expedition showed how the military can be used on missions other than waging war.

ISBN 216

188 pages

\$24.50

Gunboats and Marines: The United States Navy in China, 1925-28 by Bernard D. Cole

Investigates the role of the U.S. Asiatic Fleet in the Chinese revolution and the relationship during this period between the State and Navy departments. The fleet maintained a presence in China's coastal and riverine waters sufficient to evacuate many Americans living in China.

ISBN 203

229 pages

\$28.50

Whalemen's Paintings and Drawings: Selections from the Kendall Whaling Museum Collection

by Kenneth R. Martin

The pictorial record of American whaling as seen by its practitioners. The more than 200 illustrations, which show posterity how whalemen saw themselves and their work, are valuable not only in themselves but as historical documents.

ISBN 191

172 pages

\$30.00

University of Delaware Press

326 Hullihen Hall, Newark, Delaware 19711

Please address all orders to: 4 Cornwall Drive, East Brunswick, NJ 08816

A World of History from Yale

The Well-Ordered Police State

Social and Institutional Change through Law in the Germanies and Russia, 1600-1800
Marc Raeff

"Raeff offers stimulating reflections on the central dilemma on which the well-ordered police state rested — the need in the modern world to strike a balance between the regulative activity of government and the needs and strivings of individuals and corporate groups within society. The book is the work of a mature, erudite, and thoughtful scholar."
—Robert O. Crummey \$23.50

The Soviet Union and the Arms Race

David Holloway

"Basing his analysis on careful study of the historical experience, the policy objectives, and the institutions that have sustained the Soviet military effort, Holloway has produced a remarkably acute assessment of what drives Soviet military policy."
—McGeorge Bundy, *The New York Review of Books* \$14.95

Code Name "Mary"

Memoirs of an American Woman in the Austrian Underground
Muriel Gardiner
Foreword by Anna Freud

"Muriel Gardiner has given us a gripping account of how she, a wealthy young American studying in Vienna, came to be at the hub of underground socialist activity... No self-styled thriller can match this book's story... Should be required reading for anyone who wants to understand what it was to be an anti-fascist in Central Europe in the Thirties."
—Joseph P. Lash \$14.95

Yale University Press
New Haven and London

One Day in China: May 21, 1936

translated, edited, and introduced by Sherman Cochran and Andrew C. K. Hsieh with Janis Cochran

This version of *One Day in China*, the first ever to be published in English, contains eighty-four of the original first-hand descriptions of what it was like to live in China a half century ago.



"Offers a wondrously comprehensive overview of the Chinese people.... In terse, moving essays, these untrained Chinese writers share with us their foibles, beliefs, and prejudices in the last fragile year of peace before the Japanese war erupted." —Jonathan D. Spence
\$19.95

The Image of the Architect

Andrew Saint

A lively and wide-ranging discussion of the changing role of the architect in society. Tracing the image of the architect from the beginnings of the profession in the eighteenth century through the Bauhaus years to the present day, this is a "well-written and original piece of work." —*Library Journal* 59 illus.
\$19.95

St. Martin's Press: a primary source

Germany in the Later Middle Ages

F.R.H. Du Boulay, *the University of London*

In the medieval burgeoning of the German tongue and subsequent birth of the Teutonic national identity, Du Boulay finds the early roots of 20th century Germany and offers a detailed portrayal of the religion, monarchy, principalities and societies that came together to form the whole of later medieval Germany.

June 32625-4 220 pp. \$30.00

The Arena of Capital

Michael Dunford, *the University of Sussex*,
and Diane Perrons, *the City of London Polytechnic*

In this well-written study the authors present a comprehensive account of the historical geography of the development of capitalism in Britain from a Marxist perspective. They make an important contribution to the theory and methodology of geographical inquiry in their concise statement of the basis for Marxist study of spatial developments.

August 04857-2 400 pp. \$37.50

Alphonse de Lamartine

A Political Biography
William Fortescue, *the University of Kent*,
Canterbury

Fortescue sheds new light on the currents of French thought and opinion in the period between the Restoration Monarchy and the Second Empire within a biographical framework in this unique study of Alphonse de Lamartine, one of the major figures in the political and intellectual life of 19th century France. He draws upon a wide range of sources that include manuscript letters as well as official documents—unpublished and published—in his exploration of the evolution of Lamartine's complex personality and his literary and political careers.

April 02138-0 304 pp. \$29.95

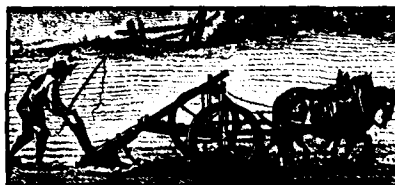
The Dynamics of Agricultural Change

The Historical Experience
David Grigg, *the University of Sheffield*

"Immensely thorough, well-organized and clearly written." E.L. Jones, *Yale University*

In this major overview of the history of agriculture, Grigg brings together a wide range of interpretations and descriptions of agricultural change—from theorists such as Karl Marx and B.H. Slicher van Bath to Malthus and von Thunen. Completely up-to-date and exhaustive in detail, this study offers a significant contribution to agricultural history and will surely be a primary text for years to come.

May 22316-1 304 pp., illus. \$26.00



Soviet Foreign Policy, 1930-1933

The Impact of the Depression
Jonathan Haslam, *the University of Birmingham*

The first of a three-volume history of Soviet foreign policy from the Great Depression until Russia's entry into World War II in 1941, this study draws on the resources of many archives and illuminates the development of international relations in Russia from 1930 to 1933, a period of massive internal upheaval—domestically as well as internationally.

August 74838-8 210 pp. \$27.50

St. Martin's ISBN prefix: 0-312-
All prices and publication dates are subject to change
without notice.

for outstanding historical studies

The Making of Modern Russia

Second edition

Lionel Kochan, the Battersea County School, and Richard Abraham

Since the first edition's publication more than 20 years ago, this study has generated much praise and has firmly established itself as the general history of the subject. In light of recent scholarship, this classic has been thoroughly revised and updated to include several entirely new chapters as well as new material dealing with such timely themes as the position of women in Soviet society, and the growing influence of Islam in the Soviet Union.

April 50703-8 250 pp. \$25.00

Russia and the Origins of the First World War

D.C.B. Lieven, the London School of Economics and Political Science

The Making of the Twentieth Century Series

This penetrating volume examines in detail the outside pressures which forced Russia into the First World War—the geopolitical, diplomatic, domestic political, military and economic factors. Besides drawing upon a wide range of archival sources—Russian emigré, Soviet, French, Australian and British—Dr. Lieven also makes extensive use of primary material published in Russia which has never been previously studied by historians.

June 69608-6 225 pp. \$25.00

National Minorities in Eastern Europe, 1848-1944

Raymond Pearson, the New University of Ulster

In this first comparative survey of the minority populations living in Eastern Europe—including Albanians, Czechs, Jews, Magyars, and Serbs—Pearson discusses their demographic origins and their growing nationalism after 1848. Illustrated with maps and tables, this historical study of one of the most complex aspects of modern Europe will provide rewarding reading for all students and scholars in the field.

July 55944-5 260 pp. \$27.50

John Calvin

edited by G.R. Potter and M. Greengrass, the University of Sheffield

Documents of Modern History Series

Using primary source materials to trace and illustrate the life and thought of John Calvin, one of the most prolific writers among the religious reformers, this authoritative volume presents carefully selected extracts from the reformer's writings (including sermons, memoranda and letters) as well as from accounts by his friend and biographer, Theodore Beza.

June 44277-7 180 pp. \$20.00

Making Ends Meet

Pawnbroking and Working-Class Credit
Melanie Tebbutt

For working class families in Victorian England, credit was frequently the only alternative available in the constant battle against inadequate financial resources. In this first comprehensive history of the pawnbroking trade, Tebbutt examines its vital—and frequently insidious—social and economic impact on working class finance.

August 50669-4 260 pp., illus. \$27.50



To order these books at a 20% discount off the list price, please send check or institutional purchase order directly to:

St. Martin's Press

Reference & Scholarly Books Division, Rm. 93
175 Fifth Avenue, New York, NY 10010

The Challenge of Change

The Sun Oil Company, 1945—1977

By Arthur M. Johnson. An examination, from the perspective of top management, of the challenges posed a major American oil company since World War II that focusses on the responses made by a corporate management that has had to cope, on the one hand, with wide swings in the availability of its basic natural resource and radically altered conditions in the international energy market, and, on the other, with policies of the federal government that increasingly affected the conditions, extent, and costs of changes management had to make in its operational, financial, and entrepreneurial strategies. \$30.00

The Social Relations of Science Movement

Great Britain, 1931—1947

By William McGucken. A history of the movement that came hesitantly into being sparked by one alarming development in international affairs—a worldwide economic depression of disastrous proportions—and propelled by others: the rise of fascism in Europe, the persecution of scientists in Nazi Germany, the illegal application of scientific technology such as the Italians' use of mustard gas against the Ethiopians, the enthusiastic cultivation of science in the Soviet Union and the alleged frustration of science in the West, and, above all, the looming and pervasive threat and eventual eruption of World War II. \$22.50

A Woman's Place

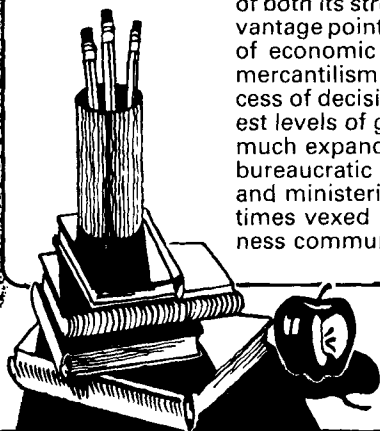
The Life History of a Rural Ohio Grandmother

By Rosemary O. Joyce. Because theoretical research is a matter more of finding the right categories than of measuring within them statistically, Joyce finds it particularly appropriate in investigating the roles of women to use the kinds of data supplied by the life history. From the rich variety of interdisciplinary perspectives that characterize her academic speciality—folkloristics—she has undertaken the life history of a remarkable woman named, for purposes of her study, Sarah Flynn Penfield—a woman who, in this penetrating analysis, becomes a paradigm for conflicts in other women who, though possessed of strong self-images, share ambivalent attitudes toward their own sex, and tend to see woman's place as more than a geographical entity. Illus. \$20.00

The French Council of Commerce, 1700—1715

A Study of Mercantilism after Colbert

By Thomas J. Schaeper. Established under the reign of Louis XIV, the Council was destined to play a crucial role in the history of France for the first decade and a half of its existence. In a detailed and comprehensive examination of both its structure and activities, Schaeper finds a fresh vantage point from which to study anew that special body of economic policies that is commonly referred to as mercantilism. He gives close attention to the actual process of decision-making as it was conducted at the highest levels of government, and is thus enabled to reach a much expanded and more precise understanding of the bureaucratic machinery that acted to enforce royal laws and ministerial decrees, and of the complex and sometimes vexed relations between the crown and the business community. \$30.00



**OHIO STATE
UNIVERSITY PRESS**
2070 NEIL AVENUE, COLUMBUS, OHIO 43210

WISDOM OF ROYAL GLORY (Kutadgu Bilig)

A Turko-Islamic Mirror for Princes

Yūsuf Khāṣṣ Hajib

Translated, with an Introduction and Notes,

by **Robert Dankoff**

Written in 1069, Wisdom of Royal Glory is the earliest literary monument of Turkish Islam. Drawing on Turkish traditions of royalty and wisdom and placing them within the framework of Islamic culture, Yusuf offered his royal patron a mirror of court life, a guide for his conduct, and advice on how to ensure the success of his rule. In turn, the work provides us with a mirror of the political and cultural life of the Central Asian Turks at the outset of their Islamic career.

Cloth \$22.00 296 pages

ETYMOLOGIES and GENEALOGIES

A Literary Anthropology of the French Middle Ages

R. Howard Bloch

Drawing upon the work of the Annales school, traditional literary scholarship, and contemporary critical theory, Bloch crosses the usual bounds between ethnology, philology, philosophy, economics, and history to establish the relation between language theory, family structure, and poetics in the High Middle Ages. A milestone in contemporary medievalism, Bloch's analysis represents a fresh point of departure for discussion of the roots of modern consciousness.

Cloth \$29.00 294 pages illus.

THE AGE OF THE CATHEDRALS

Art and Society, 980-1420

Georges Duby

Translated by **Eleanor Levisux** and **Barbara Thompson**

"A provocative and illuminating work. The author is so thoroughly in tune with his period . . . that the reader is plunged into the feelings of medieval men and thereby into the world that produced the cathedrals."

—American Historical Review

Cloth \$22.50 Paper \$9.95 318 pages 35 plates

HISTORIOGRAPHY

Ancient, Medieval, and Modern

Ernst Breisach

Breisach narrates and interprets the main lines of development of historiography in Western culture, from its origins in Greek poetry to the present. His comprehensive survey displays the richness of the discipline as it provides an understanding of the historiographical views of past generations and gives proof that history is an enduring endeavor inextricably bound to the structure of Western life.

Paper \$12.50 (est.) 416 pages

Library cloth edition also available \$30.00 (est.)

The University of Chicago Press Chicago, IL 60637

CAMBRIDGE

History in Paperback...

Never at Rest

A Biography of Isaac Newton

Richard S. Westfall

"This is the first [biography of Newton] to be based on an extensive use of the manuscript sources. It is more than merely the biography of a man and an account of his ideas. It is a fundamental resource work on the Scientific Revolution, a work that should be on the shelves of every scholar interested in the main currents of ideas of the seventeenth century or any aspects of the early history of modern science."—*American Historical Review*

\$14.95

The Newtonian Revolution

With Illustrations of the Transformation of Scientific Ideas

I. Bernard Cohen

"A typescript of I. Bernard Cohen's Wiles Lectures... has circulated among interested parties, and citations from it have appeared in numerous footnotes. Now we have the published version, a major contribution, like everything that comes from Cohen's pen, both to the understanding of Isaac Newton and to the historiography of science."—*Journal of Modern History*

\$16.95

The Foundations of Newton's Alchemy

or "The Hunting of the Greene Lyon"

Betty Jo Teeter Dobbs

"Dobbs's book should be read not only by scholars of Newton; it should be basic reading for students of the Enlightenment as well as for historians interested in the connection between Renaissance Neoplatonism, broadly interpreted, and the origins of modern thought."—*The Eighteenth Century*

\$16.95

The Renaissance Notion of Woman

A Study in the Fortunes of Scholasticism and Medical Science in European Intellectual Life

Ian Maclean

"It adds immeasurably to the rigor, depth, and scholarship of the argument about women and provides remarkable insight into the connections among the disciplines in a century of proliferating academic production."

—*Renaissance Quarterly*

\$9.95

Mystical Bedlam

Madness, Anxiety, and Healing in Seventeenth-Century England

Michael MacDonald

"The history of psychiatry... in the eighteenth and nineteenth centuries has been well served. By comparison, that of the two previous centuries is a *terra incognita*... We are, therefore, deeply indebted to Professor MacDonald for charting this academic wasteland with such commendable skill. His book is an example of scholarship at its best."—*Journal of the Royal Society of Medicine*

\$14.95

A Liberal Descent

Victorian Historians and the English Past

John W. Burrow

"Mr. Burrow has taken a closer look at... the so-called 'Whig interpretation'... no summary can possibly do justice to the richness, subtlety, and originality of this book, to my mind one of the finest volumes to have appeared since his own *Evolution and Society*..."—John Clive in *The New York Review of Books*

\$14.95

The Rise of the New Model Army

Mark A. Kishlansky

"This important book is less a study of the New Model Army than an exposition of its author's now celebrated thesis about 'adversary politics'... Kishlansky's admirable account [supplies] some important new elements in both fact and interpretation which no historian of the Great Rebellion can afford to ignore..."—*Journal of Modern History*

\$14.95

Liberalism and Sociology

L. T. Hobhouse and Political Argument in England 1880–1914

Stefan Collini

"The first full examination of the new liberalism as a problem in intellectual history... adds an important dimension to this debate... A short review cannot do justice to this fine work, a major contribution to the intellectual history of the period."—*American Historical Review*

\$14.95

The Beginning of Ideology

Consciousness and Society in the French Reformation

Donald R. Kelley

"His purpose is to attempt to reconstruct aspects of the experience, thought and behaviour of articulate French people... one cannot deny [its] subtlety and pertinence... to our understanding of sixteenth-century French religious and political thought."—*Journal of Ecclesiastical History*

\$14.95

Capitalism and the State in Modern France

Renovation and Economic Management in the Twentieth Century

Richard F. Kuisel

"I believe that Kuisel's work will be received as the most important book on French history since Stanley Hoffmann's *In Search of France* (1963). And the significance of Kuisel's work goes beyond French history, since it deals with the changing nature of capitalism and shows how such changes can be expressed in historical terms."—Martin Wolfe

\$14.95

Cambridge University Press

The Edinburgh Building, Shaftesbury Road, Cambridge CB2 2RU

32 East 57th Street, New York, N.Y. 10022

"When we have the completed work we shall have one of the major achievements of British historiography in this century."

—J.M. ROBERTS, THE DAILY TELEGRAPH

William Pitt the Younger *The Reluctant Transition*

John Ehrman. This is the second volume of a definitive three-volume biography of William Pitt, Prime Minister of England from 1783 to 1801 and from 1804 until his death in 1806. It centers on the middle years of his career—from 1789 and the aftermath of the Regency crisis to 1796 and the end of the first wartime coalition—and shows the brilliant young Minister of a successful peacetime decade grappling with one of the most prolonged crises his country had ever known. Illus. \$39.50

The first volume in the series: WILLIAM PITT THE YOUNGER: THE YEARS OF ACCLAIM. Illus., 1969. \$45.00

Flowers in Salt *The Beginnings of Feminist Consciousness in Modern Japan*

Sharon L. Sievers. This book traces the roots of Japanese feminism to women's struggle for individual rights and new political, social, and economic roles in the changing society that followed the Meiji Restoration of 1868. The author concentrates on those women who were outspoken critics of their society and the roles women were assigned in it, but also assesses the contribution women made to Japan during a period of rapid modernization. Illus. \$22.50

Order from your bookstore, please



Stanford University Press

Alba

A Biography of Fernando Alvarez de Toledo,
Third Duke of Alba (1507-1582)

WILLIAM S. MALTBY

Fernando Alvarez de Toledo was one of the most powerful figures of his day. He was involved in virtually every political decision of the mid-sixteenth century, but is best known for his six tragic years as ruler of the Netherlands under Philip II. To some a hero, to others a cruel fanatic, he has personified the sterner aspects of Spain's golden age. Maltby presents a vivid story of a man whose pursuit of virtue led to tragedy, and in so doing illuminates a crucial period in the history of Spain, the Netherlands, and Europe as a whole. \$29.50, illustrated

The Powers of Prophecy

The Cedar of Lebanon Vision from the Mongol Onslaught to
the Dawn of the Enlightenment

ROBERT E. LERNER

Lerner traces the fortunes of an eschatological prophecy that was first written around 1240 and thereafter circulated throughout Western Europe for more than four centuries. He explores hitherto neglected aspects of medieval mentalities and demonstrates the importance of prophecy and chiliasm as medieval means for coping with the instabilities of the present and the uncertainties of the future. \$32.50

China Among Equals

The Middle Kingdom and Its Neighbors, 10th-14th Centuries

Edited by MORRIS ROSSABI

These essays challenge the conventional wisdom that China devised its world order and maintained it from the second century B.C. to the 19th century. The authors affirm, rather, that China did not dogmatically enforce its own world order from the 10th to the 13th centuries. \$28.50 hardcover, \$12.50 paperback

The Birth of Vietnam

KEITH WELLER TAYLOR

Taylor appeals to both Chinese and Vietnamese sources to obtain a balanced view of the birth of Vietnam—from the beginning of recorded history in the 3rd century B.C. until the 10th century, when Chinese control ended and an independent Vietnamese kingdom was established. \$38.50

Italian Marxism

PAUL PICCONE

Piccone mandates significant revisions in the history of Marxism, and raises questions about the relevance of any type of modernized Western Marxism. He shows that the inconsistencies in the Italian Communist Party, since its return to legality after World War II, are rooted in the reception of Marxism by Labriola, Croce, and eventually Gramsci. \$24.95

1 8 9 3 Available at bookstores or order from

90

UNIVERSITY OF CALIFORNIA PRESS
BERKELEY 94720

1 9 8 3 Ninety years of scholarly excellence

Recent Pamphlets

AMERICAN HISTORICAL ASSOCIATION

AHA PAMPHLETS—narrative and critical essays, including bibliographical guides, on topics in history

- 101 The American Frontier Thesis: Attack and Defense *by Ray Allen Billington*
102 American Intellectual History: The Development of the Discipline *by Arthur A. Ekirch, Jr.*
212 The Progressive Era, 1900–20: The Reform Persuasion *by George E. Mowry*
215 Contemporary American History: The United States since 1945 *by Dewey W. Grantham*
222 Far Western Frontiers *by Harvey L. Carter*
240 The Indian in American History *by William T. Hagan*
241 The Peopling of America: Perspectives on Immigration *by Franklin D. Scott*
250 A History of the American Labor Movement *by Albert A. Blum*
260 Religion in America: History and Historiography *by Edwin S. Gaustad*
702 American Diplomatic History in Transformation *by Alexander DeConde*

DISCUSSIONS ON TEACHING—essays on approaches to history in the classroom

- 2 Teaching History with Film *by John E. O'Connor and Martin A. Jackson*

Pamphlets are \$1.50 each; payment must accompany order. A complete list of titles is available upon request.

The American Historical Association
Pamphlet Orders A21
400 A Street, SE
Washington, DC 20003

PLEASE SEND TO:

NAME (PLEASE PRINT) _____

ADDRESS _____

ZIP CODE _____

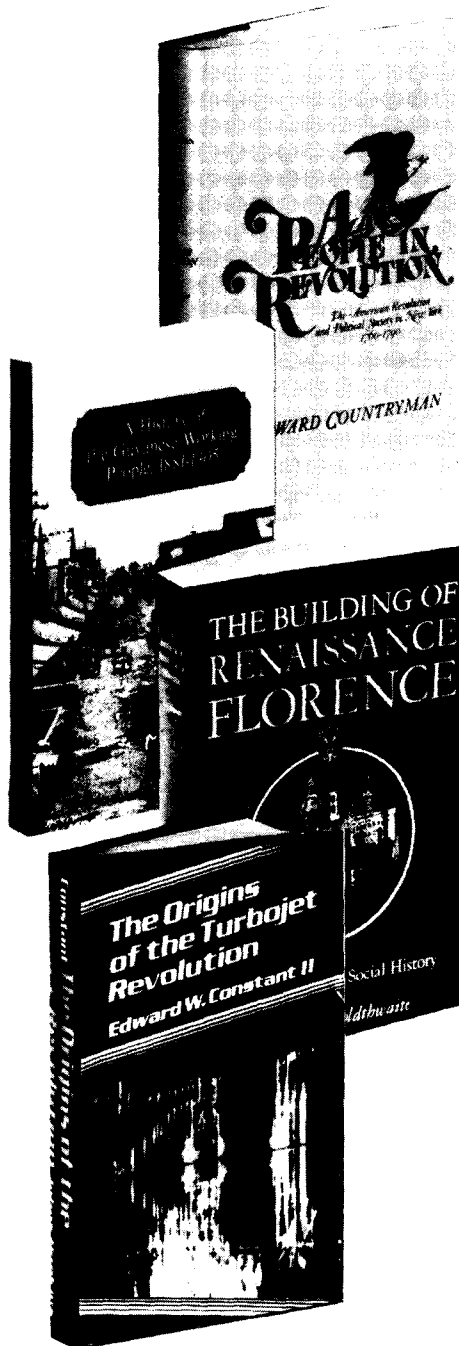
I enclose \$_____ (check or money order; no stamps please) in payment for the _____
pamphlets indicated below:

AHA PAMPHLETS 101____ 102____ 212____ 215____ 222____ 240____ 241____ 250____
260____ 702____

DISCUSSIONS ON TEACHING 2____

Please send complete list of titles_____

Johns Hopkins Publishes Winners!



**THE JOHNS HOPKINS
UNIVERSITY PRESS**

Baltimore, Maryland 21218

Bancroft Prize

A People in Revolution

EDWARD COUNTRYMAN

"...deserves to be read by every serious student of the American Revolution." —

James Kirby Martin, Journal of American History

An account of the struggle for independence in New York State and the internal reorganization that continued long after the war ended.

\$24.50 hardcover

Awarded the 1981 Bancroft Prize of Columbia University for exceptional merit and distinction in American history.

Beveridge Award

A History of the Guyanese Working People, 1881-1905

WALTER RODNEY

"Rodney... wrote... to make his ideas—and history—entirely intelligible to the people about whom and for whom, he wrote." —

Sidney W. Mintz, New York Times Book Review

A landmark investigation of nineteenth-century Guyanese society.

\$6.95 paperback, **\$26.50** hardcover

Winner of the 1982 Albert J. Beveridge Award of the American Historical Association for the best book on the history of the United States, Canada, or Latin America.

Marraro Prize

The Building of Renaissance Florence

RICHARD A. GOLDTHWAITE

"...a more lively and more authentic impression of life in Renaissance Florence." —

Felix Gilbert, New York Review of Books

A complete picture of fifteenth-century Florentine society in dynamic transformation.

\$10.95 paperback, **\$29.50** hardcover

Awarded the 1981 Howard R. Marraro Prize of the American Historical Association for the best scholarly study on Italian history.

Dexter Prize

The Origins of the Turbo-Jet Revolution

EDWARD W. CONSTANT II

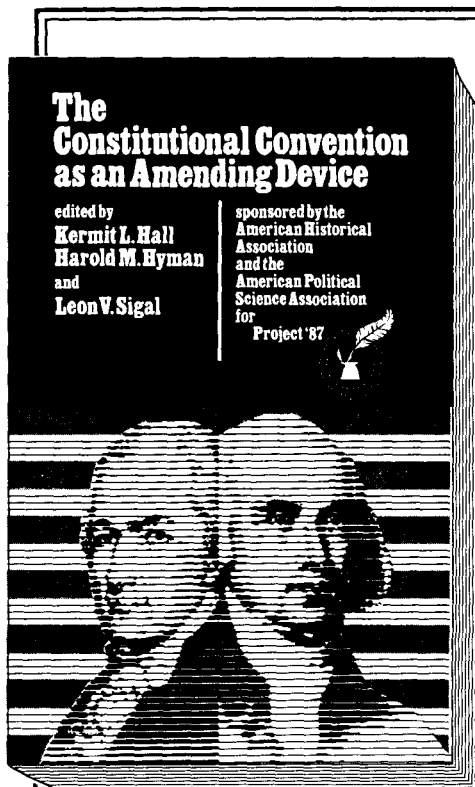
"...an impressive and solid work of scholarship... certain to become a standard reference work on the subject." —

Richard P. Hallion, Science

The first full-length study of the technology that revolutionized modern aviation.

\$24.00 hardcover

Awarded the 1982 Dexter Prize of the Society for the History of Technology for outstanding scholarship in the field of the history of technology.



A Timely Study from Project '87

**Essays on
Constitutional
Change
and
Constitutional
Conventions**

Order Form

Please send () copies at \$6.50 ea. for paper
() copies at \$10.50 ea. for hardcover

of **The Constitutional Convention as an Amending Device**

to: Name _____

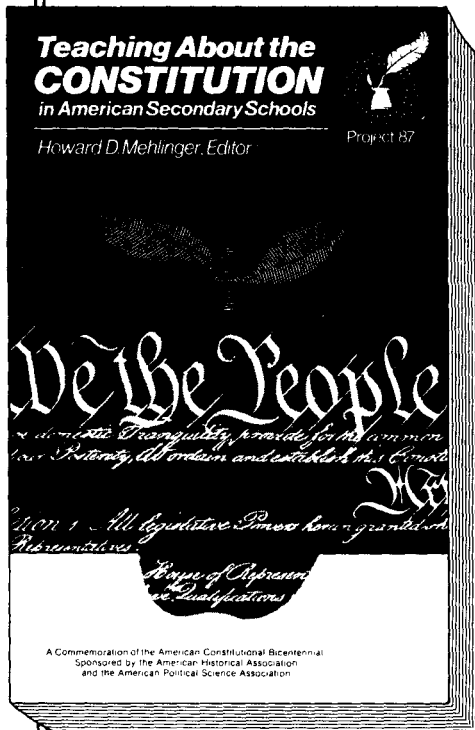
Address _____

City _____ State _____ Zip _____

Order from: **Project '87/AHA**
400 A Street S.E., Washington, D.C. 20003

For a full list of available publications, write to:

American Historical Association, Publications Department
400 A Street S.E., Washington, D.C. 20003



A Report from Project '87's Conference on Secondary Education and the U.S. Constitution

Order Form

Please send () copies at \$6.50 ea. for paper
() copies at \$10.50 ea. for hardcover

of **Teaching About the Constitution in American
Secondary Schools**

to: NAME _____

ADDRESS _____

CITY _____ STATE _____ ZIP _____

Order from: **Project '87/AHA**
400 A Street S.E., Washington, D.C. 20003

For a full list of available publications, write to:

American Historical Association, Publications Department
400 A Street S.E., Washington, D.C. 20003



If you are planning to move, please let us know six weeks before changing your address. Attach address label and fill in your name and new address below. This will ensure prompt service on your subscription.

Attach Label Here
(address label found on AHR wrapper) Send label with your name and new address to American Historical Association, Membership Department, 400 A St., S.E., Washington, D. C. 20003. If a label is not available, be sure to attach your OLD address, including Zip Code number.

Name _____

New address _____

City _____

State _____

Zip _____

Teaching Women's History

This new AHA pamphlet, written by Professor Gerda Lerner, provides a much-needed overview of the field, women's history. Lerner discusses various methods and techniques for teaching women's history. An essential work for anyone interested in this expanding subject. Bibliographical notes included. Price: AHA members \$4.00, nonmembers \$5.00.

Recent United States Scholarship on the History of Women

Presented at the Fifteenth International Congress of Historical Sciences, this work is designed to illuminate the important contributions made by historians of women and published during the last fifteen years. It is also an attempt to assess the direction and achievement of the field. Authors: Barbara Sicherman, E. William Monter, Joan Wallach Scott, Kathryn Kish Sklar. Price: AHA members \$3.50, nonmembers \$4.50.

A Survival Manual for Women (and Other) Historians

An updated version of a pamphlet that first appeared in 1975. The AHA Committee on Women Historians decided that unwritten rules often lead to inequity; so the CWH set out to reveal the most important rules and customs. Price: AHA members \$4.00, nonmembers \$5.00.

Guide to Departments of History, 1982-83

Includes listings of close to 370 U.S. and Canadian departments (of which 135 grant the PhD) and twelve research institutions. Price: AHA members \$15.00, nonmembers \$20.00.

Grants and Fellowships of Interest to Historians, 1982-83

Includes more than 190 entries. Funding programs are divided into three categories: support for dissertation and postdoctoral research; support for predissertation study and research; support for organizations working in the field of historical research and education. Book awards and prizes are also listed in this edition. Price: AHA members \$4.00, nonmembers \$5.00.

The Past Before Us: Contemporary Historical Writing in the United States

Edited, with an Introduction, by Michael Kammen. Foreword by John Hope Franklin. "A detailed, up-to-date, insider's guide to the current activities of the historical profession: who does what, with what and to whom."—*Washington Post Book World*. "A document of major importance."—*The N.Y. Times Book Review*. (Sponsored by the American Historical Association.) Published by Cornell University Press, \$19.95.

For a full list of available publications, write to:

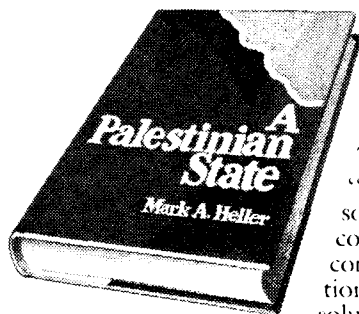
Publications Department
400 A Street SE
Washington, DC 20003

All orders must be prepaid; include \$1.00 for shipping and handling. (*The Past Before Us* available at bookstores or direct from Cornell University Press, P.O. Box 250, Ithaca, NY 14850)

Index of Advertisers

American Historical Association	20, 22–25	St. Martin's Press	12–13
Basic Books, Inc.	3	Stanford University Press	18
Cambridge University Press	16–17	University of California Press	19
Harvard University Press	Cover 3	University of Chicago Press	15
Johns Hopkins University Press	21	University of Delaware Press	10
Louisiana State University Press	5	University of Nebraska Press	4
Ohio State University Press	14	University Press of New England	8
Oxford University Press	Cover 2	University Press of Virginia	9
Princeton University Press	6–7, Cover 4	Yale University Press	11

**If you oppose the
creation of a
Palestinian state,
this book
could change
your mind.**



A Palestinian State

The Implications for Israel

Mark A. Heller,

*Center for Strategic Studies,
Tel Aviv University*

“Superb . . . a model of how to do a sophisticated policy analysis of a highly complex political-security issue. It should compel a full reconsideration of their positions by those who espouse alternative solutions.” —Samuel P. Huntington

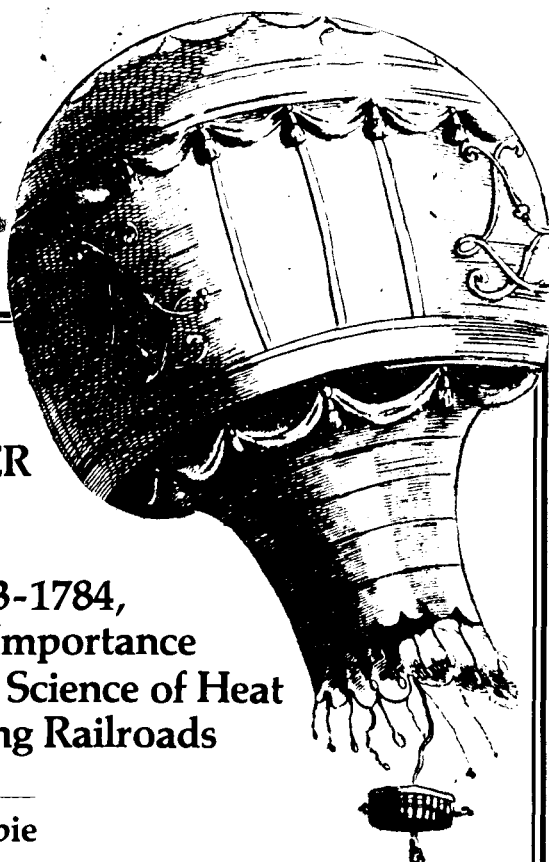
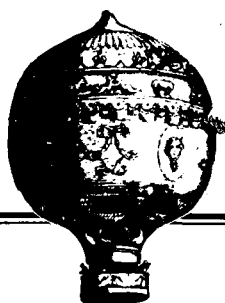
“There is nothing in the literature comparable to what Mark Heller has done . . . His analysis is informed by a deep firsthand knowledge of Palestinian and Israeli societies, and a thorough grounding in the relevant historical, geographic, strategic, and military literature.” —Nadav Safran

“Dispassionate and scholarly . . . an important addition to any discourse on the problems of the Middle East, not only because of its rich sources (including actual interviews in the West Bank), but also because of Heller’s insightful analysis and lucid presentation.” —Joel S. Migdal

\$16.00 at bookstores

Harvard University Press

Cambridge, Massachusetts 02138



**THE MONTGOLFIER
BROTHERS AND
THE INVENTION
OF AVIATION, 1783-1784,
With a Word on the Importance
of Ballooning for the Science of Heat
and the Art of Building Railroads**

Charles Coulston Gillispie

"The best book published to date on the invention of the balloon, this work is a case study illuminating some very serious scientific issues and at the same time a very readable narrative treatment of an extraordinarily romantic subject."—*Thomas Crouch, Curator of Aeronautics, National Air and Space Museum*

"Get in a supply of taffeta and cordage quickly," wrote Joseph Montgolfier to his younger brother, Etienne, in November 1782, "and you will see one of the most astonishing sights in the world." In a vividly illustrated narrative of the Jules Verne-like adventures that followed Joseph's summons, Charles Coulston Gillispie introduces the readers to the brothers Montgolfier, who launched the first hot-air balloon in Annonay, France on 4 June 1783. Drawing on unexploited family papers, he portrays the innovating Montgolfiers not only as important contributors to technology and to the beginnings of aviation but as characters in a highly entertaining tale of 18th century dare and imagination.

90 illus., 11 in color. 8½ x 11". \$35.00

Princeton University Press

41 William Street, Princeton, NJ 08540